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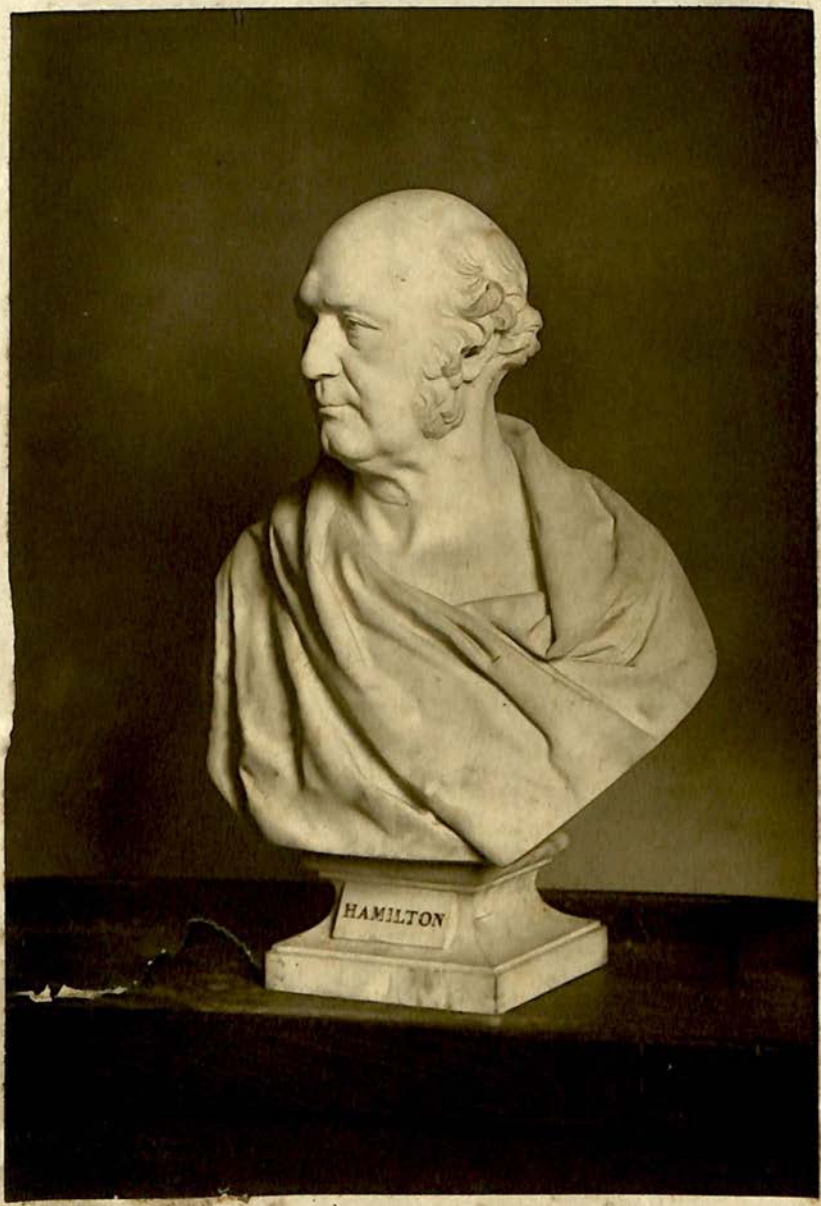
HAMILTON

DUBLIN UNIVERSITY PRESS SERIES.

LIFE

OF

SIR WILLIAM ROWAN HAMILTON,

KNT., LL.D., D.C.L., M.R.I.A.,

ANDREWS PROFESSOR OF ASTRONOMY IN THE UNIVERSITY OF DUBLIN,
AND ROYAL ASTRONOMER OF IRELAND, ETC. ETC.:

INCLUDING

SELECTIONS FROM HIS POEMS, CORRESPONDENCE, AND MISCELLANEOUS WRITINGS.

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BY

ROBERT PERCEVAL GRAVES, M.A.,

SUB-DEAN OF THE CHAPEL ROYAL, DUBLIN,
AND FORMERLY CURATE IN CHARGE OF WINDERMERE.

VOL. III.

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PREFACE.

THE biographical portion of this volume was brought to a close and printed early in 1887. This fact will account for occasional references in it to persons and events which are not in exact accord with the present state of things. The subsequent and larger portion of the volume consists of a selection from a very extensive correspondence between Sir W. R. Hamilton and Professor Augustus De Morgan. To make this selection has involved the expenditure of much time and labour. The quantity of material was so great that I have had to exclude matter that possessed inherent value, either because it was in subject unsuited to this work, or because, being mathematical, the investigations carried on were too abstruse or too extended. The general reader will perhaps complain that I have introduced more than enough of mathematical investigation; but he will, I hope, withdraw this complaint when he calls to mind that it was as scientific men that the writers corresponded, that it would be unjust to them if their correspondence as printed should not retain this character, and that the mathematical discussions did in fact most often afford suggestion to the play of thought which, passing beyond the boundaries of science, prompted the wit and the learned and pleasant gossip which all readers will enjoy. Its possession of the elements last mentioned was the circumstance which determined me to place this correspondence in immediate connexion with the biography. In some instances I have given a condensed statement of the mathematical matter under discussion. For

such statements I have, in important cases, been indebted to the scientific knowledge of Mr. G. L. Cathcart, M. A., F. T. C. D., who has also by his advice and information saved me from printing what had either ceased to be of interest to the scientific reader, or had been afterwards published by the writers themselves. I cannot but take this opportunity of publicly expressing my gratitude to Mr. Cathcart for a service from which the readers of the correspondence will derive much benefit, and which cost him a sensible addition to the labours of his well-occupied days.

Another great service to me and to this work demands my ample acknowledgment, although that acknowledgment cannot now be received by her who had so amply earned it. Miss Emily Anne Napier, of whom I feel it an honour to have been a friend and distant kinsman, voluntarily undertook to perfect for me the chronological arrangement from the beginning of the original letters of Sir W. R. Hamilton and his friends, and to make year-lists of them, noting the principal contents of each letter. This task, calling for great accuracy, method, and perseverance, was fully accomplished by her, notwithstanding interruptions from illness and from many other calls of friendship and benevolence. It was not unlike the service rendered by her admirable Mother to her Father, the Historian of the Peninsular War, and I am deeply sensible of the extent of the obligation under which it has laid me.

To my friend Dr. Ingram, S.F.T.C.D., I must here express renewed thanks for continuing to this volume the invaluable advantage which my work has derived from his revision of the proof-sheets, and from the indication of errors detected by his wide and accurate knowledge.

In the first volume a note was made of the fact that I was indebted to a friend for the Index appended to it. I feel now at liberty to state that the friend referred to was Mrs. Edward Dowden, and in thus rendering her my thanks for a valuable service spontaneously offered, I am happy to have the

opportunity of mentioning that in the course of my labours I have received unfailing assistance from her distinguished husband, whenever I was at a loss as to a poetical allusion, or as to any point connected with literature.

The Index of the present, as of the second volume, has been carefully prepared by Mr. T. W. Lyster, B.A., of the National Library of Ireland, to whom I am also indebted for most efficient aid in drawing up the List of Sir W. R. Hamilton's works, and of publications connected with them—a list which will be found more complete than that of the Royal Society.

I should fail to perform an act of justice were I not, in regard to Mr. G. Weldrick, of the University Printing Office, and his principal assistant Mr. Rooney, to acknowledge gratefully the invariable attention I have received from them, and the care they have taken in directing the printing of this work. Nor ought I to omit my thanks to the Readers and Compositors engaged upon it for the pains they have taken to secure its freedom from typographical errors. All have worked as if they were taking a willing part in honouring the memory of an illustrious countryman; and, although misprints, inevitable in any large work, and particularly in one introducing extensively mathematical formulæ, are to be met with in these volumes, it is trusted that they may fairly claim credit for a more than average correctness of typography.

Lastly, I renew the expression of my heartfelt obligation to the Board of Trinity College for their liberality in taking upon themselves the expense of publishing this work, intended to be a full record of the scientific achievements, the varied attainments, and the personal character, of an eminent alumnus of the University of Dublin, and also for their grant made at the beginning of my undertaking of the sum of £100 towards the payment of amanuenses employed by me during the course of my work.

Of the imperfections of that work, resulting from my not being a mathematician, and from other deficiencies, I am fully

conscious; but I commit it to the public in the confidence that its materials, being the authentic documents of the minds of Hamilton and his friends, will constitute it an enduring monument of the man as well as of the mathematician, and in thankfulness to the Source of all blessings that I have been enabled, through a long period not unbroken by illness and trial, to carry on to the end my humble part in building it up.

R. P. G.

1, Winton Road, Leeson Park, Dublin,
March 9, 1889.

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From	Date of Letter.	
	1841.	
A. DeM.	May 8.	Presents his Paper *On the Foundation of Algebra* as introduction. Asks for Triplets.
W. R. H.	May 12.	Algebra the Science of Pure Time. Kant. P.S.—Triplets, Polyplets.
	1842.	
A. DeM.	Jan. 3.	Hansen. Logometers. Divergent Series.
W. R. H.	Jan. 28.	Second Essay on Dynamics.
A. DeM.	Jan. 31.	Invites Report on Hansen.
W. R. H.	Feb. 8.	Investigations on a different line from Hansen's. Fluctuating Functions.
	1844	
A. DeM.	Oct. 11.	Abstract of a Paper on Triple Algebra.

From	Date of Letter.	
W. R. H.	Dec. 9.	Does not refuse to triplicise. Challenges any other theory of Pure Triplets to surpass in three points that included in his Quaternions. Symmetricalness of Space the most important. 1st P.S.—Solution of problem as to resultant of forces, &c. 2nd P.S.—Argument against Triplets reconsidered. 3rd P.S.—Would not prevent the study of Pure Triplets.
A. DeM.	Dec. 16.	You all for interpretation. My object to keep to symbolic rules. Suspects you have the right sow by the ear.
,,	Dec. 30.	Fractional Differential Coefficients. Cautions as to health and work.
	1845.	
W. R. H.	Jan. 5.	Two notes. Geometrical interpretation of A. DeM.'s Cubic System.
A. DeM.	Jan. 12.	Acknowledgment.
,,	Dec. 23.	Asks for information as to H.'s *quarto* publications. Advises to give correct information to bibliographers.
	1846.	Mr. (C.) Graves's Paper. H.'s Symbolic Geometry.
W. R. H.	Feb. 2.	Quaternions "a new algebraical geometry." Intention to retire from Presidentship of R. I. A. Time of Easter. Equinox in the year of the Council of Nicæa.
A. DeM.	Feb. 15.	C. Graves's Paper a finished thing. Quaternions a system in which rotations play the part of co-ordinates. Approves of resignation of Presidentship. Easter. Burckhardt. Chernac's *Cribrum Arithmeticum*. Mistake of Gregorian Committee. Arbogast's method of derivations.
	1847.	
,,	April 12.	Appeals from a charge of piracy in Logic brought against him by Sir W. Hamilton of Edinburgh. The two Sir W. Hamiltons constantly confounded.
W. R. H.	May 7.	*Researches respecting Quaternions*. Manner in which the mathematical notion of *Time* leads to a *general* conception of Numerical Sets. Mind on the present subject less analytical than synthetical. "Geometrical Fraction" of one set of Papers the same as "Quaternion" of another. Offers Paper on Law of Circular Hodograph. Interested at present in some applications of Quaternions to Physical Astronomy.
,,	July 10.	Death of his Uncle James. Coincidence. Hopes that Quaternions will simplify physical astronomy.
A. DeM.	July 12.	Is there a *mental dynamics*? Action of Moonlight. Stories of ghosts. No doubt of the applicability of quaternions.
	1848.	
,,	Aug. 17.	Appeal for Second Part of Memoir on Dynamics.
W. R. H.	Aug. 21.	Feels that he ought to write a quaternionic treatise on mechanics. Pressed to write a book on Quaternions.

From	Date of Letter.	
	1849.	
A. DeM.	April 28.	Mr. Wentworth Erck. Co-cosine. Solution of $x^3-2x=5$ to 103 decimal places.
,,	Aug. 30.	Professor Young's loss of his post at Belfast.
W. R. H.	Sept. 24.	Professor Young. Commissioner Hargreave. Peacock's observation that Functions and Quaternions have several analogies. Infinitely many solutions of $\rho^2=-1$.
A. DeM.	Sept. 26.	Professor Young. Hargreave. Algebra itself a functional calculus. Some loose thoughts (algebraical). Parentheses. Chancery suit. Swing-swang integration.
W. R. H.	Oct. 3.	Answer to one of the "loose thoughts." Mr. Hargreave.
A. DeM.	Oct. 5.	"Closed circuit" question. Illustration of boys and beadle.
W. R. H.	Oct. 10.	Some electrical phenomena of circuits not unlikely to correspond with A. De M.'s law. Theorem respecting Gauche Polygons. Curious to see a demonstration of either theorem by co-ordinates.
A. DeM.	Oct. 11.	A searcher for things mental not material. Pascal's theorem about the hexagon.
,,	Nov. 12.	An enigma for W. R. H.'s daughter.
	1850.	
,,	April 6.	*Relatives.* Motto of *Cambridge and Dublin Mathematical Journal.*
W. R. H.	April 9.	Generalisation of A. De M.'s theorem.
A. D. M.	April 14.	[Two Notes.] The two Sir. W. Hamiltons reciprocal polars. More on the theorem.
W. R. H.	April 18.	Continued discussion of the same theorem.
,,	Oct. 19.	The Calculus of Quaternions includes all ordinary Algebra. Points in which Algebra and Quaternions are distinguished.
	1851.	
,,	Nov. 26,	Thanks for Paper on "Recent Discoveries on the Invention of Fluxions." Vindication of Leibnitz from charge of plagiarism gives him pleasure. Suspects he has been plagiarised from. Book on Quaternions advancing rapidly. Asks for references to works having connexion with the subject of quaternions. Relations between Double Algebra and Quaternions.
A. DeM.	Nov. 26.	Reason for shrinking from the subject of Triplets. Death of F. Baily. Sir W. Hamilton of Edinburgh. Quaternions must contain the double algebra. Plagiarism. Omission in published list of W.R.H.'s Time paper. Wheatstone an authority on German writers. Leibnitz. Laplace.
W. R. H.	Dec. 1.	Use of $B-A$ in geometry. Wheatstone.
A. DeM.	Dec. 4.	Acknowledgment.

From	Date of Letter.	
W. R. H.	Dec. 8.	Asks for criticism of the metaphysics of the early articles of his *Lectures*.
,,	Dec. 10.	Object of *Lectures*. A fatal fault in Double Algebra.
A. DeM.	Dec. 15.	Acknowledgment.
W. R. H.	Dec. 16.	Despairs of writing anything good on the *history* connected with quaternions.
A. DeM.	Dec. 17.	Will set you at rest about the history.
W. R. H.	Dec. 18.	Fears a mistake in a quotation from Persius. Walker's Logic.
A. DeM.	Dec. 20.	Quotation correct. Scientific poems.
W. R. H.	Dec. 23.	Walker's Logic. John Walker.
,,	Dec. 24.	Has in the last year composed only four sonnets. Could never write verses on the death of a blood-relation.
,,	Dec. 24.	Depôt. French accents. Greek accents. English spelling.
A. DeM.	Dec. 27.	Thanks for Sonnets on *Tetractys* and *Garden*. John Walker. Depôt. Accents. Peacock's Report on Analysis. Gauss. About 1831–33 there was a *revival* of pure algebraical speculation. The generalities of Sir W. R. H.'s paper incapable of being transferred elsewhere. Special features of quaternions.
	1852.	
W. R. H.	Jan. 3.	Polyplets. Geometry at the last moulded and fixed my conception of ijk, yet abstract speculation led to it.
A. DeM.	Jan. 4.	Find Quaternions no child's play. All that is peculiar to Quaternions beyond the reach of anyone except yourself to claim. Nothing in Gauss to suggest quaternions. Ohm on Algebra, translated by Ellis. Give definitions. Your alleged defect in double algebra a defect in symbols.
W. R. H.	Jan. 5.	Encloses order for Ohm's book.
,,	Jan. 6.	Private reason for knowing that Gauss had not anticipated quaternions. Baron von Walterhausen.
,,	Jan. 7.	Acknowledgment. Up all night for eclipse.
A. DeM.	Jan. 7.	Mathematicians and amusements. Mathematical booksellers. Gauss's *Disquisitiones Arithmeticæ*. J. Walker. Gauss's Quadratic residues.
W. R. H.	Jan. 8.	Style of *Lectures on Quaternions*. Do not assume much previous knowledge of mathematics.
,,	Jan. 10.	Gauss's quadratic residues. Solution by quaternions of the topical problem of Apollonius.
A. DeM.	Jan. 10.	Lends Mourey. French likely to claim quaternions for Mourey.
W. R. H.	Jan. 13.	Servois nearer than Mourey to such an anticipation. Argand. Français. Buée. P. S.—Mourey's proof of the existence of a root. History of $\sqrt{-1}$.

From	Date of Letter.	
A. DeM.	Jan. 15.	Criticism of Servois. Mourey nothing else but a part of double algebra.
,,	Jan. 18.	Criticism of Sonnets. Functions of $\frac{0}{0}$ in common algebra. Instance. History of $\sqrt{-1}$.
W. R. H.	Jan. 21.	Combination of science and imagination personified in Herschel. Careless of criticism of my poems. Sensitive as to my mathematics.
A. DeM.	Jan. 22.	Geodetic lines observed in Dublin Observatory. Asks for description for Annual report of Astronomical Society. Priority question fully discussed.
W. R. H.	Jan. 26.	Hopes as to Quaternions, and opinion of their present state. Gauss's biquadratic residues. Kummer.
,,	Jan. 26.	Origin of the word "Panic" from Polyænus.
,,	Jan. 27.	Change of mode of address. Lady Campbell. Euclid in Greek. Bishop Butler's observation about caution to avoid being mistaken.
A. DeM.	Jan. 28.	Pan's horns.
W. R. H.	Jan. 31.	Suppresses suspicion as to plagiarism on himself. Generous treatment by eminent men. Explanation as to himself and the Graveses in relation to De Morgan and Triplets. Mac Cullagh and Quaternions. Polyænus's *Stratagems*. Once invented a Lucianic dialogue about the Rape of Helen. "Reviewers Reviewed."
A. DeM.	Feb. 2.	Suppose your race susceptible as to priority. His own nativity. Mulcahy on the altitudes of modern geometry. Murphy. Salmon. Geometry taken root in Ireland. Suspicion of others copying inferentially.
W. R. H.	Feb. 9.	Proof of a theorem of Desargues. Pure geometry in Ireland. Mulcahy. Eton and Learning. Irish susceptibility. Prejudices against Quaternions. Anecdote. Mac Cullagh's question. Salmon.
A. DeM.	Feb. 12.	The equilateral hyperbola. The ellipse. Modern geometry worked on a system of *signs*. Salmon's working of general propositions. Ridiculers of Quaternions. Mac Cullagh's question. Objects to phrase "Six points in involution."
W. R. H.	Feb. 13.	De Morgan "On the Connexion of Involute and Evolute in Space." Ball at the Castle. Allman, Jukes, Anster, Lady Kane.
,,	Feb. 14.	Euclid. Thales. The equilateral hyperboloid. Comment on De Morgan's Paper "On the Connexion, &c." The circle not a degenerate ellipse, but an ellipse a deformed circle.

From	Date of Letter.	
A. DeM.	Feb. 15.	The Circle and the Ellipse. *Self-similarity* in both. Also in the *Screw*, which it was a failure of Euclid not to introduce. Cylinder. Ring. Self-cutting Ring. Incloses note to Mr. Salmon—really a valuable writer. Lady Kane.
W. R. H.	Feb. 21.	Mr. Salmon. [Letter from Mr. Salmon of Feb. 24: Proof of anharmonic theorem, readiness to employ the most convenient instrumentality. Quaternions now a bow of Ulysses which no one can bend but the owner. Reply by W. R. H.: Proposes comparative exhibition of different mathematical methods.]
A. DeM.	Feb. 23.	A table of contents a palisade against mere talkers. Irish more given to pure speculation than the English.
W. R. H.	Feb. 25.	Difficulty in printing.
,,	Mar. 15.	Is the word "eliminant" your invention?
,,	April 2.	Speech in honour of Moore. His "rebellious" poetry. No harm in it.
,,	April 15.	Mesmerism. Miss Edgeworth, Herschel. Believing one's eyes. Conical Refraction. Dumas' "Mémoires d'un Médecin." Lord Clarendon. Babbage.
A. DeM.	April 15.	Grant's *History of Physical Science*. Differential Calculus. Laplace. Fermat.
W. R. H.	April 16.	Southey's saying about proof-sheets. Sydney Hamilton of Trim, infant pupil in Sanscrit and Persian. Mathematical examination papers.
A. DeM.	April 16.	Clairvoyance. Good notion of fluxions from Oresmius, Episcop. Lexov.
W.R.H.	*April 19, 20.	Walter Scott on extraordinary events. Herschel's 'Collection of Examples.' A theorem. Astrology. Algebra of bi-couples.
,,	*April 22.	Definition of the differential of a function of a quaternion. Equation in quaternions representing an arbitrary surface of revolution.
A. DeM.	*April 23.	Comment on definition of differential. Suggests another mode of differentiating a function. Will require a year after book is out to follow you. Sir W. Hamilton of Edinburgh. 'Rowan.' Astrology. *Guy Mannering*. Alexandrian Sorcerer.
W. R. H.	*April 26.	Comment on A. De M.'s method of differentiating a function. Challenge. Three advantages of his own method.
A. DeM.	*April 27.	Sceptical about $(a+b)^{\frac{1}{2}}$, &c. A difficulty. When both m and n are fractional what possible form of development?
W. R. H.	*April 29.	No possible form of development analogous to the usual one. In quaternions there is a development.

* Letters with an Asterisk are to be found at pages 623-632.

From	Date of Letter.	
A. DeM.	*April 30.	Doubts about $\phi^{\frac{m}{n}}$. There is a development, but which of an infinite number?
,,	*May 1.	Reply to yours of April 19. Cannot interpret W. Scott on extraordinary events. Instances of coincidences. Astrology. $n^2 - n$ intruding roots. Cardan's result, page 623.
,,	May 3.	Geometry and its algebra in T. C. D. Punishment for it in the next world.
W. R. H.	May 3.	Determination of the differential of the square-root of a quaternion.
,,	May 5.	Priority. Proposes to cite DeM. Not pleased with Bronwin's citation of 'Hamilton's Theorem.' 'Curse of Kehama.' Latin poem by Leibnitz.
A. DeM.	May 6.	Walker could *think* about logic. My own claim. Partial interpretations may solve problems. 'Hamilton's Theorem,' Herschel's. The extension Hamilton's. Citation. Letter to Sir W. H. of Edinburgh. ὑποστασις, οὐσια, χαρακτηρ. Heb. i. 3.
W. R. H.	May 8.	Heb. i. 3. Walker's Logic.
,,	May 9.	Henry Crabb Robinson. Herschel's Theorem?
A. DeM.	May 10.	Herschel's Theorem. Heb. i. 3. Intends to apply the theory of probabilities to St. Paul's alleged authorship of this Epistle.
W. R. H.	May 12.	To whom are divine honours to be paid? Regrets having entered into religious controversy.
A. DeM.	May 16.	De Morgan not de Morgan. Reason for not taking M.A. degree. Worship of Jesus Christ, meaning of 'worship.' 'Honour.' Anecdote. *Porismatic system.* Libri.
,,	May 21.	$\sqrt{-1}$ of multiple interpretation. Whewell's name for interrupted inversion. Wanted a short and sound interpretation of a theorem. Fermat's Method of maxima and minima. Kepler.
W. R. H.	May 24.	Multiplicity of interpretation of $\left(\frac{d}{dx}\right)^{\frac{m}{n}}$. DeM.'s proof of a cited equation satisfactory.
,,	May 26.	Possibility of a visit to Greenwich. In 1838 climbed into the Ball at St. Paul's. Effect of the wind. Coincidence with an expression of C. Dickens. Another coincidence with an expression of Tennyson. Dinner-rhymes. Biquaternions.
A. DeM.	May 27.	Will not be at Greenwich. Dinner-rhymes. Cannot keep to Swift's vein. First surgical operation under mesmerism.

* Letters with an Asterisk are to be found at pages 623-632.

From	Date of Letter.	
A. DeM.	May 31.	Shadow of a pole at sunset.
W. R. H.	June 2.	A new sort of variations of definite integrals. Cauchy. Libri. Fellowship Examinations conducted in English.
A. DeM.	June 8, 9.	$\phi x = 0$. Partial solutions of equations. Algebra not made for geometry. Double algebra is of a surface, single algebra linear, double areal. Quaternions with the three rotations ought to be the triple algebra.
W. R. H.	June 12.	Results respecting a particular sort of variation of a definite integral. Investigations respecting differentials of functions of quaternions more general than that respecting the differential of a square-root.
A. DeM.	June 15.	Men to whom *Lectures on Quaternions* should be sent.
,,	June 20.	Dr. Logan. Libri. Kirkman's mathematical mnemonics. Catechism of mathematics. Libraries. Airy, prince of *method*ists.
W. R. H.	June 23.	Expectations in connexion with Quaternions as regards electricity, magnetism. Polarity. Mnemonics. A formula hard to be remembered. Politics.
A. DeM.	July 1.	Lodges a paper on Syllogism considered as a composition of relations. A play on words.
,,	July 11.	Oppressive heat. Chasles: *Géométrie Supérieure*. Liouville's Monge. Poinsot's Theory of Rotation. Justin Brenan *v.* Logic.
W. R. H.	July 12.	Dublin election. Beautiful aspect of city and river. The Bi-tensor of a product of Biquaternions.
A. DeM.	July 14.	Politics. My reform of the House of Commons. Theory of decision by votes. House of Lords. In English hands, not objecting to a moderate mixture of Irish and Scotch, anything would work well. Macaulay on the House of Commons. Libri. Prince Boncompagni. Gerard of Cremona, Translator of Ptolemy. Negative quantity in Arabic algebra.
,,	July 16, 17.	$\infty = 10^{13}$. Reinhold's *Tabulæ Directionum*. My Mnemonic for the hard-to-be-remembered formula. Suggests a sonnet. Libri.
W. R. H.	July 20.	Politics. Robert Hutton. T. Drummond. The Ballot.
A. DeM.	July 23.	The *Times* on Libri. Jewels of criminal law in France. Prof. Young. Maynooth.
W. R. H.	July 23.	Dublin politics.
,,	July 26.	Admits deep prejudice in politics. Reminiscences. Claim of family descent. Maynooth.
A. DeM.	July 27.	Politics. How I would tinker the Constitution. Evil of subscriptions. Opposition to Maynooth grant founded on a subversive principle. Reinhold's *Tabula fœcunda*.

From	Date of Letter.	
W. R. H.	July 28.	Against multiplying oaths. Bribery.
A. DeM.	July 31.	Epigram on the Duke of Norfolk and curry powder. Your Seal. Lucas Pacioli's *Summa di Arithmetica*. The last day of letters prepaid in money.
W. R. H.	Aug. 3, 4.	Calculation unfavourable to the notion that the asteroids are fragments of an old planet. Such calculations not his most useful employment. His birthday and his younger son's.
,,	Aug. 8.	Lithograph of C. Wolfe's autograph of 'Not a drum was heard.' Preface. First or Third Person? Lady H. and his daughter desire to see De Morgan.
A. DeM.	Aug. 9.	Asteroids probably parts of a *ring*. Bibliography. Errors in Description.
,,	Aug. 10.	Reasons for First Person. Advice as to Preface. Scotch Ancestry. Dr. Parr to Sir James Mackintosh.
W. R. H.	Aug. 11.	Two Solutions of your puzzle for my daughter. Her birthday.
,,	Aug. 13.	Chasles on imaginaries. Time of publication of book.
A. DeM.	Aug. 16.	Only two solutions of the puzzle. Dates.
W. R. H.	Aug. 20.	Paper on the Ascension. As Trinitarian as Athanasius. Sermon by Salmon.
A. DeM.	Aug. 23.	F. Baily's correspondence. Your handwriting diminishing. Your Trinitarianism. Your 'sermon.' Mr. Robinson and belief in the Devil. Mrs. Flamsteed's Valentine.
W. R. H.	Aug. 26.	Ely Sonnet. Sir J. Herschel's to Hamilton.
A. DeM.	Aug. 30.	The Wolfe lithograph. A Peacock à Phœnix. The Three Churches in the Ely Sonnet. Herschel a man of poetical elements.
W. R. H.	Sep. 1.	Explanation of Ely Sonnet. Bishop Terrot. Abp. Whately and the memorial window in Castleknock church.
A. DeM.	Sep. 1.	Bad practice of printers putting books a year forward. Theology. An agreeable lull.
W. R. H.	Sep. 2.	Satisfaction about the Wolfe autograph. My Preface a bore.
A. DeM.	Sep. 3.	*Paradise Lost* necessarily Tritheistic or Arian. Whately and emblems. I would not abolish superstition and mysticism. My notions about burial.
W. R. H.	Sep. 16, 17.	Belfast Meeting of British Association. Carlingford. Renewed interest in Quaternions.
A. DeM.	Sep. 20.	Given X-land, what constitutes an X-man? Chancellorship of Oxford. Walton, opponent of Berkeley's *Analyst*: son of Isaak W.?
W. R. H.	Sep. 25.	Return to Dublin.
A. DeM.	Sep. 27.	Portfolios. History of infinitesimals in England. Leibnitz. Newton.

From	Date of Letter.	
W. R. H.	Sep. 29.	Salmon on Temporal Blessings. National Sins and Judgments. Irish murders. Gift of a Douay Bible. Charitable supposition of R. C. Priest.
A. DeM.	Sep. 29.	Enquires for Boole about Dublin printers. Check upon printers.
,,	Oct. 1.	Salmon's argument that religion was intended to develop the inquisitive faculties sound, but involves the consequence that differences of opinion were also intended. A difference with Salmon. Irish Exodus —pun. Answer to application for subscription towards a R. C. purpose.
W. R. H.	Oct. 3.	Dublin University Press. Boole.
,,	Oct. 4.	Bishop Berkeley. Walton. 'Fluctions' and 'fluxions.'
A. DeM.	Oct. 5.	Irish bulls. Boole has got hold of the true connexion of algebra and logic.
W. R. H.	Oct. 8.	Gauche Polygons.
A. DeM.	Oct. 23.	Editions of Berkeley. Infinitesimals in England. Newton. Four colours will colour any possible map.
W. R. H.	Oct. 26.	Asks for a sketch of the history of Double Algebra. $\sqrt{-1}$. Grassmann.
A. DeM.	Oct. 29.	Cannot comply with request. Grassmann (Nebuchadnezzar). Sir W. H. of Edinburgh and his charge about Ploucquet. Translate for me your German.
W. R. H.	Oct. 30.	Begun Preface-writing.
A. DeM.	Nov. 8.	Series and mean values.
W. R. H.	Nov. 16, 18.	Triplets worked at in 1835. Right now to record this in Preface.
A. DeM.	Nov. 20.	Admits his right. Not at the Duke's funeral. The procession.
W. R. H.	Nov. 24, 25.	Your Triple Algebra and my Triplets. Fundamental difference between them. My notion of triplets as old as 1830.
A. DeM.	Dec. 15.	Do not be uneasy about priority. Grass and mutton.
W. R. H.	Dec. 18.	Admits nervousness as to anticipated controversy. When it comes the vexation is over. Trisection of an angle. Is it impossible by Euclid? Gauss.
A. DeM.	Dec. 24.	Your priority-vexations a reflection on Lady Hamilton. Proposed resolutions of British Association. Gauss's discovery of a method of inscribing a polygon of seventeen sides increases my disbelief in possibility of trisecting an angle. Tells a story.
	1853.	
W. R. H.	Jan. 8.	Argand true author of Double Algebra, not Buée. Asks for any information to the contrary relative to Buée.
,,	Jan. 26.	Double Algebra. Buée, De Morgan, Wallis.
A. DeM.	Jan. 27.	Buée, Wallis, Warren. Am afraid of virtual discoveries.

From	Date of Letter.	
W. R. H.	Jan. 28, 31.	J. T. Graves confirms recollections about Argand. Buée, Warren, Wallis. Been reading Grassmann with admiration and interest. Surprising that Grassmann failed to hit off Quaternions.
,,	Feb. 1.	Verification of recollections about Argand. Not quite so enthusiastic to-day about Grassmann. Comparison of his own work and Grassmann's.
A. DeM.	Feb. 5.	Take a side, and care for nobody. Arago and the watch. Willing to be mentioned favourably. Skin so thick it can neither be pierced nor tickled. Thirtieth anniversary of my starting for Cambridge.
W. R. H.	Feb. 9.	Comparison of work with Grassmann's. Argand did not anticipate De Morgan. Extract from Latin poem of Leibnitz.
,,	Feb. 11.	Theorem suggested by hints from Cotes and Herschel.
A. DeM.	Feb. 12.	German intellect. Tobacco-smoke. Difficulties of extension to experts used to it. D. F. Gregory. Sir W. H. of Edinburgh cannot abstract quantity. Leibnitz.
,,	April 1.	Inquiries.
W. R. H.	April 4.	Returned from fairyland, an investigation in the application of bi-quaternions to the inscription of polygons in surfaces. This day would have been birthday of Sister, who *was* a poet. Claims for himself only poetical temperament and feeling.
A. DeM.	April 14.	Objects to his limitation of his own pretensions to poetry. Corrects expression about birthday. All opposite terms in logic come under *plus* and *minus*. *Universal* –, *particular* +, &c. Recommends change of occupation from quaternions. A four-course succession.
W. R. H.	May 5.	Illness of children.
A. DeM.	May 9.	Table rapping. Our thoughts are read.
,,	May 21.	Poetry runs in families more than mathematics. Lady Lovelace and the Differential Calculus. Verses from Morgan's *Horologiographia*, and Hylles' Arithmetic.
W. R. H.	June 30.	Feelings after printing off a book. Anxiety about a word. Observations at Dunsink quinquisected by Royal Commissioners. Mr. Charles Thompson.
A. DeM.	July 2.	A new Double Algebra. The two Sir W. H.'s, + 1 and − 1. Argand's lamp. Rotation of intellectual crops.
W. R. H.	July 7.	A new symbol of operation, $\mathrm{I}_{\frac{1}{2}}$.
A. DeM.	July 11.	Your new subject not recreation. The sailor's three wishes. Differential equations.
W. R. H.	July 16.	Book (*Lectures on Quaternions*) actually out.
A. DeM.	July 18.	Punning suggestion of letters to be added to Sir W. R. H.'s name.
W. R. H.	July 25.	Method of least squares and theory of probabilities. Suspects fallacy in himself and Encke.

From	Date of Letter.	
A. DeM.	July 29.	Disputes 'both you and Encke.'
W.R.H.	July 29, 30.	Agrees with DeM. as to fallacies.
,,	Aug. 18.	Suggests abridgment in De Morgan's argument (*Diff. and Int. Calc.*) as to theorem of mean value. Error in Moigno's *Leçons de Calcul Différential.*
A. DeM.	Aug. 19.	Admits difficulty. Homersham Cox. The principle of fluxions always gives light.
,,	Aug. 22, 23.	Problem in probabilities as to set of books. Imperfect cerebration.
W.R.H.	Aug. 22, 24.	Inverse Probabilities. The Comet.
A. DeM.	Aug. 26.	Mrs. DeM.'s sight of the comet with naked eye. Thundering error in my Probabilities. Everyone makes errors in probabilities. Laplace, Poisson. Illustration.
W. R. H.	Sept. 7.	A theorem of Laplace. Royal visit to Dublin. The previous one of 1849. The first Great Exhibition in Dublin. Interview with the Prince Consort.
A. DeM.	Sept. 12.	Presents tract on Probabilities. My thundering error. The theorem of Laplace. What I have *not* seen.
W. R. H.	Sept. 28.	British Association Meeting at Hull. Beverley Minster and Flamborough Cliffs. Phillips and Sedgwick. York. Bolton Abbey. Belfast. Carlingford.
A. DeM.	Oct. 4.	Guild of Literature and Art. C. Dickens. F. Baily's handwriting. Lord Brougham and Quaternions. Conundrums.
W. R. H.	Oct. 31.	Death of a friend. Guild of Literature and Art.
A. DeM.	Dec. 10.	Invention of Double Algebra.
W. R. H.	Dec. 15.	Telegraphic experiments in connexion with his second son. His cipher. Dr. Hincks.
,,	Dec. 17.	Daughters.
	1854.	
A. DeM.	Jan. 10.	Death of his daughter. Horner's Method, and its author.
W. R. H.	Feb. 20.	Horner's method. Secretary Drummond. Solution of a linear and partial differential equation in bi-quaternions. The existence of *two* arbitrary functions a startling departure from the analogy of the older algebra. Carmichael.
A. DeM.	Mar. 4.	Arbitrary functions queer fishes: example.
W. R. H.	Mar. 7.	A transformation of the integrals involving only one arbitrary function.
,,	Mar. 14.	Mathematical blunders of Auguste Comte. His capacity of admiration.
A. DeM.	Mar. 26.	Comte's verbosity and self-sufficiency. His 'final system of Knowledge.'
,,	April 18.	Dupin's proof (regarding the lines of curvature on a system of orthogonal surfaces) diabolical. The

From	Date of Letter.	
		words 'synthetical' and 'analytical.' Derivation of name of Easter.
W. R. H.	April 24.	Dupin's genius. Le bon diable. De Morgan a Hippopotamus.
A. DeM.	April 29.	Dupin a decent devil in mathematics. Intends to write letters of advice to young mathematicians, with a Hippopotamus on the title page.
W. R. H.	April 25.	Quaternions a Calculus.
A. DeM.	April 29.	Quaternions certainly a Calculus. My course of teaching. Infinitesimals.
W.R.H.	May 12, 20.	Calculation with quaternions, geometrical interpretation in the background. Simplest way of proving by calculation the modular theorem.
,,	May 25.	The Electro-magnetic Quaternion. (Quaternions furnish a CALCULUS OF POLARITIES).
,,	May 27.	Withdraws the epithet Electro-magnetic as assuming too much. Dr. Lloyd.
A. DeM.	July 10.	Rule for giving an extempore lecture. Stevinus. Joseph Fenn's 'Instructions, &c.', at the Dublin Society, 1772, uses notation of the differential calculus.
W. R. H.	July 14.	Mourey's proof that every algebraic equation has a root. Versors, tensors. A triplet-system.
,,	Aug. 11.	Cipher. Asks De Morgan if he would be stakeholder.
A. DeM.	Aug. 12.	Anything but decipherer. Tables of quarter squares.
	1855.	The Potato.
W. R. H.	Jan. 3.	Payment by Board of Printer's bill. 'Extensions of Quaternions.' Would require some time to read. Easy to *you*.
A. DeM.	Jan. 6.	A shorter demonstration is time saved in teaching. Example of arguing in a circle. Double algebra and quaternions.
W. R. H.	Jan. 9.	Find I can understand my own researches on Dynamics. Connected this method with quaternions in 1845.
A. DeM	Jan. 15.	A remonstrance. Read an old paper of my own not knowing its author: pleased with it and gratified by this testimony to myself.
,,	April 29.	A fundamental method of Newton generally unknown. Gives the problems. Carmichael on the Calculus of Operations. Who is he? Rouse out from the quaternions.
W. R. H.	May 2, 4.	Up all night with the moon.—'He' or 'It' of an infant. A 'young *pagan*.' Mrs. Wilde: her son Oscar. Her remark on the Observatory. More sociable.
A. DeM.	May 16.	'It' for a baby. 'Shortened.' Sponsors' undertakings.
W. R. H.	July 11.	Decimal coinage: Mr. Lowe. Maria Edgeworth's pun. Her age.
A. DeM.	July 13.	Puns. Pitching into a joker a good service. Jokes on serious subjects.

From	Date of Letter.	
A. DeM.	Sept. 5.	Refers to his own article on Brewster's *Newton*. Lying fallow. Sheepshanks. Ballads: Curiosities in the history of popular ballads. Simplicity an ultimate attainment. Euclid I. 47. Kelland. Airy.
W. R. H.	Oct. 2, 24.	Glasgow. Jessop on Decimal System. 'Morning Register.' Extensions of Quaternions.
A. DeM.	Oct. 29.	The 'Fine Old Irish Gentleman.' Decimal Coinage: Lord Monteagle, Article in *National Review*. Mr. Jessop. Galileo. Anti-Romanist joke.
W. R. H.	Nov. 1.	Decimal Coinage. Abduction of Miss Arbuthnot.
A. DeM.	Nov. 4.	The abduction case. English legal-headedness.
	1856.	
,,	Mar. 3.	The words of the 'Fine Old Irish Gentleman.' Additional stanza by De Morgan.
W. R. H.	June 11.	A protest against omission of his name from Grant's *History of Physical Astronomy*. Jacobi. Donkin. Cayley. Houel.
A. DeM.	Dec. 26.	Death of Sir W. Hamilton of Edinburgh. Obituary notice by A. DeM. The creed of the Decimal Association. Isonosical lines.
	1857.	
,,	Jan. 12.	Air of 'Irish Gent.' in A. DeM.'s musical notation. *Note*. Petrie on this notation.
,,	Jun. 18.	Sir W. H. of E. on Mathematics.
W. R. H.	Mar. 22.	Early days at Glasnevin. Varenii *Geographia Generalis* edited by Isaac Newton. Instance of historical inaccuracy on a recent and public occurrence.
A. DeM.	May 4.	Newton's Varenius. Story for story. What is history? Libri.
W. R. H.	May 20.	Salmon and Quaternions.
A. DeM.	June 15.	Salmon's name. Your son. *Affirmative* and *negative* as applied to propositions: definition must be enlarged.
W. R. H.	June 24.	The *Times*. Its powers of face.
A. DeM.	July 3.	Bulk of the *Times* staff Irish. Newspapers mirrors.
,,	Aug. 3.	Quaternions not for ladies. Approves W. R. H.'s Decimal Coinage Bill. Archibald Smith. Preserving letters. Specimen of my folly in a letter to Mansel. Wills. Logics and Mathematics.
W. R. H.	Aug. 7.	Terms with Salmon. With Mac Cullagh. Notion of writing a Tract on Cones of the Second Order treated by Quaternions. Asks for opinion.
A. DeM.	Aug. 14.	Opinion as to such a book.
W. R. H.	Aug. 14, 17.	Abandons the project. Notion of another tract. Letters to Salmon. British Association.
A. DeM.	Aug. 21.	British Association. Am not in sympathy with physical philosophers. Object to travelling.
,,	Nov. 15.	What curves cannot include a space? No news to tell.

From	Date of Letter.	
W.R.H.	Nov. 16, Dec. 4.	Definite Integrals and Transformation of Series. Theorems (A) (B) (C) (D) (E).
A. DeM.	Nov. 20.	Cauchy's 'principal value.' Curves that do not inclose space: elementary proof. Proof that every equation has a root.
W. R. H.	Nov. 23.	Cauchy's proof. Mourey's method. Writing merely to free the mind mutually allowed. Professor Stokes on Definite Integrals. A correction.
A. DeM.	Nov. 27.	Every convex spherical polygon has its supplemental polygon. Cauchy's proof. Mourey's. Argand. Airy and $\sqrt{-1}$. Herschel on a certain discord.
W. R. H.	Nov. 28.	Correcting misprints. Definite Integrals.
A. DeM.	Dec. 6.	Airy's paper on $\sqrt{-1}$. 'In all haste.'
W. R. H.	Dec. 18.	G. Stokes on my Definite Integrals.
A. DeM.	Dec. 19.	Your last letters [on Definite Integrals] a memoir in themselves.
W. R. H.	Dec. 14.	Transition from Double to Quadruple Algebra.
,,	Dec. 22.	Herschel's wit *in re* Decimal Coinage.
,,	Dec. 23.	A convert to Divergents.
	1858.	
A. DeM.	Jan. 1.	Comment on letter of Dec. 14. Vindication of Cauchy. The French and Divergent Series. Four sorts of Series.
W. R. H.	Jan. 4.	Cauchy, admire his mathematical talents. My investigations by quaternions respecting *families of surfaces* the germ of a new Calculus of Partial Differentials. Great importance of 'alternating series.' Where Mourey succeeded, where he failed.
A. DeM.	Feb. 20.	The logicians too mathematical. Organ-tuning by beats. March 1. Pascal's Theorem. Projection. Apollonius.
W. R. H.	Mar. 3.	Pascal's Theorem. Moebius.
A. DeM.	Mar. 7.	Projection. Pascal's Theorem the fundamental instrument.
,,	Mar. 23.	The police and the Trinity College lads. A shillaly tournament.
W. R. H.	Mar. 30.	Your construction of hyperbola derivable from Pascal's Theorem, which I have considered fundamental. Pascal above Chasles in inventive genius.
A. DeM.	April 1.	Newton's equation. Chanting it. Whewell's letter-puzzle.
W.R.H.	April 4, 6, 10.	Cipher. Series for $f(x+h)$ with a foreign and arbitrary constant. Silver wedding. A formula of Laplace analogous to mine.
A. DeM.	April 11.	Anticipation of your theorem by Murphy. Anticipation of life.

From	Date of Letter.	
W. R. H.	April 14.	Admits anticipation by Murphy of one of his theorems. A more general series.
A. DeM.	May 16.	The two sides of Logic, mathematical and metaphysical.
W. R. H.	May 26.	Announces finishing and despatch of his long letter on Definite Integrals.
A. DeM.	June 3.	Read through your 72-page letter: 'a collection of memoirs, a real inroad into the territory of divergent series.' *Alternation* safety in series, not *convergency*. Motto from Hobbes for my tract on Logics and Mathematics. Sir W. Hamilton of Edinburgh and 'quantity.'
,,	July 27.	The two Sir William Hamiltons. Logic of the word 'of.' Anagrams. Newton and Huyghens. Syllogism implying relation in copula. A near relation. London air. Logical examples. P. M. A. C. F.
W.R.H.	Sept. 13, 14.	Visit to Trim. Daughter dangerously ill. Dr. and Mrs. Butler. Oct. 13. My differentials what Newton would have called *fluxions*.
	1859.	
,,	Jan. 3.	Additional conclusions respecting Definite Integrals. Hon. Mrs. Ward.
,,	Mar. 11.	Monthly Notices of Royal Astronomical Society. Reports?
A. DeM.	Mar. 27.	Do not write much in Reports. Main: Donati.
,,	No date of time.	New date of place. Chalcot Villas. J. Smith and circle-squaring. Your short proof. Daughter's restoration to health. Advice to her.
W. R. H.	Oct. 9.	A plea for Italics and Capitals. Duty to Insurance Offices.
A. DeM.	Oct. 11.	Insurance Offices: a distinction as to duty towards them. Elimination. Transition. Illustrations.
W. R. H.	Dec. 1.	Deference to your criticisms.
	1860.	
A. DeM.	May 26.	An outcry for information as to himself. Report I shall circulate. Where was Sam Johnson in 1745-6?
W. R. H.	July 9.	Touring in Wicklow.
A. DeM.	Nov. 6.	Received your letter (of October 29 on Geometrical Nets in Space). Reviewing Sir W. H. of Edinburgh in *Athenæum*. He certainly never read Euclid I. 1. to understand it. C. Dickens knew it better.
,,	Dec. 18.	Glad the affair (with Tait) is so well arranged. My controversy with Mansel. Very good opponents. Bad practice of establishing exact science by quotations.
	1861.	
,,	April 29.	Tried pleurisy. Sir W. H. and 'some at least.'
W. R. H.	May 1.	Gout does not disturb my mind. TEOTA. Lady Hamilton wants to see A. DeM.

From	Date of Letter.	
A. DeM.	May 31.	Mr. Smith wants more killing. Convincing Mr. Smith. Father O'Flaherty's pig. A jingle. Lady Hamilton would be disappointed in seeing me. Another Lady Hamilton terribly bored by me. Mr. James Smith coming to London. Rhyme to Timbuctoo. Pun on Airy.
W. R. H.	June 30.	My daughter's remark on my posting in one evening notes to two Lady Hamiltons. 'Geometrical sighs.' Challenge to De Morgan. A puzzle from him.
,,	Aug. 10.	My daughter pleasantly angry. Irish Church Canon.
A. DeM.	Aug. 12.	The anger will cool. Delight in defying control: example.
W. R. H.	Nov. 9.	Compares the *Lectures* with the unfinished *Elements*.
	1862.	
A. DeM.	Mar. 6.	Inferential non-affirmation.
W. R. H.	Mar. 8.	Berkeley's criticism on remarks of Newton. Newton not consistent in his *philosophy*. I stick to the *finite quantities*. Thomas Simpson.
A. DeM.	Mar. 11.	Newton shuffling. My autograph for Miss Helen.
W. R. H.	Mar. 14.	Have adopted Finite Differentials.
A. DeM.	Mar. 15.	Differentials. Lacroix. Legendre. Limits.
W. R. H.	Mar. 18.	Dispute your negative. Lagrange unreasonably objects to modern use of the word *limit*. My differentials not infinitesimals. Philosophy of the Infinite. Science of the Infinite.
A. DeM.	Mar. 23.	Is differential to be used for infinite multiple of a differential. Supports his negative. Suggests the word *primo-rationals* instead of differentials: also terms for $\frac{k}{h}$, k, and h.
W. R. H.	Mar. 25.	Is quasi universal usage in favour of considering a differential as an infinitesimal? Lacroix. Peacock. Cauchy. Moigno. When was 'Differential' naturalised? Euler.
A. DeM.	April 1, 7.	Ranges the authorities on both sides of the differential question. Date of introduction of Diff. Calc. into Cambridge. x rate of y.
W. R. H.	April 19.	Berkeley in earnest against fluxions as well as against matter.
A. DeM.	April 20.	Infinitesimals before Leibnitz. Newton got hint of fluxions from intensionists.
W. R. H.	April 22.	x rate of y won't serve me. Infinitesimals may be introduced into quaternions. The only question as to differentials. Weight of authority on your side.
A. DeM.	April 24.	Hart's rectilinearity. A joke. My objection to your practice as to differentials. *Hey diddle diddle*, Græcè partim, partim Latinè.

From	Date of Letter.	
W. R. H.	July 2.	Differentials of quaternions. I treat them as infinitesimals in integration.
A. DeM.	June 4.	Change of local date in verse.
W. R. H.	Sept. 15.	Röber's inscription of a heptagon in a circle. Horner's method.
A. DeM.	Sept. 18, 23, 29.	Horner. Young. Hair-compasses. *Rheticus* of Pitiscus. Cavendish. *Involution* and other articles in *English Cyclopædia*. Conics or *quarics*. Infinity: twice as many pints as quarts in infinite space?
	1863.	Amosgepotically.
,,	April 3.	Infinitely small quantities?
W. R. H.	April 16.	Line at infinity in given plane. Plane and circle at infinity in space.
A. DeM.	April 26.	Expediency of waiting. Cambridge Undergraduates. Infinity a light to guide.
,,	July 31.	Collecting my queer books. Whewell's apothegm.
,,	Oct. 21.	Boyer's contracted multiplication. Short methods don't pay. Mr. Weller.
W. R. H.	Oct. 23.	Those can use who don't want it.
A. DeM.	Nov. 10.	Col. Oakes's rule for multiplication.
W. R. H.	Oct. 24.	Horner's Method. Complex Evolution? Complex Involution.
A. DeM.	Oct. 28.	Evolution and Involution. Teachers of calculation. James Smith. 666. Cabbala.
W. R. H.	Nov. 10.	Cotes's *Harmonia Mensurarum*. RATIO. A duplicate ratio. Briggs. Napier. Modulus. Modular Angle.
A. DeM.	Nov. 14.	Cotes probably not intentionally unjust to Napier. Napier did not use Naperian logs. Object to Halley's and Cotes's *Mensura rationis*. Cotes and Newton infinitesimalists. Newton a momentarian.
W. R. H.	Nov. 17.	Napier's 'Canon Mirificus.' Importance of distinguishing between Ratio and Quotient. A Quaternion, geometrically considered, a Quotient of Vectors, but do not choose to call it a Ratio of Vectors.
A. DeM.	Dec. 31.	Ratio and Quotient. Duplicate ratio. A riddle. Two papers. Another riddle. Shakespeare tercentenary.
	1864.	*Note.*
W. R. H.	June 8.	Logic of two Controversialists, Roman Catholic and Protestant. Father Tom.
A. DeM.	June 22.	The two controversialists. Middle term in a syllogism. Anti-root. Signal-factor.
W. R. H.	June 24.	The controversialists. Anti-root. Signal-factor: Argand, Peacock. Double Algebra interprets too much. Algebraic geometry requires imaginaries. Expected once to find in Quaternions a new set of geometrical imaginaries. No such instance.

From	Date of Letter.	
A. De M.	June 26.	Case in which both middles may be particular. Double algebra one of the perfect common algebras. Quaternions one of the triple algebras.
W. R. H.	July 4.	Vision. Habitually a double-seer. Binocular vision not necessary. Amount of obligation to *touch*. Visible distance. Do we believe we see the very trees?
A. De M.	July 17.	Strictly unocular. Not aware of difference from a short-sighted person with two eyes. Obligation to *touch*. *Can* draw a straight line and circles.
W. R. H.	July 19.	Your 'Unocular' a right correction of my 'monocular.' 'Homofocal,' 'Confocal.'
A. De M.	July 31.	Seeing the very objects. So words for objects. Distance recognisable without touch. A second eye would add to quantity of nervous action. Illustration. Peroxide of hydrogen.
W. R. H.	July 4.	Hamilton of E. on Reid. Interested in Hamilton's Philosophy.
A. De M.	July 9.	Hamilton of E. interesting even in Logic when quantity is not in question. At Oxford. Originality of thought of putting sign of quantity to the predicate.
W. R. H.	July 9.	Agree more with Ingleby than with Hamilton as to Perception of an External Universe. Axioms ascribed to Euclid probably not genuine. Sir W. Hamilton's view of Philosophy differs essentially from mine.
A. De M.	Sept. 22.	The Declaration of Scientific men, Herschel's stinging answer.
W. R. H.	Sept. 23, 30.	Herschel's intention in refusing to sign. Sir John Bowring.
A. De M.	Oct. 2.	Herschel objects to Declaration as mischievous. Declaration translated into English. Illustration. Cui bono. Herschel not a timid man.
,,	Dec. 13.	Pension for Professor Boole's wife and daughters. Influence of the Irish in the House of Commons. Acknowledgment of kindness.

APPENDIX.

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Two popular accounts of the Quaternion Calculus :

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2. Elementary Sketch, ... 635

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Maedler's Central Sun, ... 637

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CORRIGENDA.

VOLUME III.

THE reader is particularly requested to make the following corrections in the *Ætats* at the top of the pages specified:—

Pages 51, 53, 55, . . . *for* 50 *read* 51.
„ 83, ... „ 51 „ 52.
„ 117, 119, ... „ 53 „ 54.
„ 127, 129, 131, 133, „ 54 „ 55.
„ 135, 137, 139, . . „ 55 „ 56.
„ 141, 143, . . . „ 55 „ 57.

Page 7, line 22, *for* latter *read* later.
„ 7, note, *after* '*supra*' *insert* Vol. II.
„ 60, line 21, *for* deserters *read* dissenters.
„ 69, l. 25, *after* integers *for* comma *insert* —.
„ 135, l. 14, *for* Charles Mansfield *read* Clements Mansfield.
„ 379, l. 17, *for* Havil, Great Southampton-street, *read* Havil-street, Southampton-street.
„ 472, l. 21 *after* function *insert* Ω,.
„ 592, l. 26, *for* October *read* November.

[Minor errors are left to the intelligence of the reader].

LIFE

OF

SIR WILLIAM ROWAN HAMILTON.

CHAPTER XXXIX.

THE DARGLE. LIVERPOOL MEETING. VISIT TO WESTMORLAND.

(1854.)

THE last preceding chapter of this work recorded the publication, ten years after he had discovered the Calculus, of Sir William R. Hamilton's *Lectures on Quaternions*, and the reception by him from his scientific brethren of most gratifying testimonies to the value of the work. Both the publication and these results of it must have been an immense relief to Hamilton's mind and spirits. And indeed that relief had been greatly needed; for, as may be remembered, his intellect had been long continuously tasked, and he had passed through a period disturbed by much care and emotion. Early in the year 1854 he received from an old friend a letter of thanks for a presentation copy of the book, which, though containing a disclaimer of the ability to read and judge of it, could scarcely have been less welcome to the author than homage from the highest mathematical authority. Its writer was Professor Sedgwick, the veteran geologist, who in it expresses, in his own natural and vigorous style, his affectionate feelings towards Hamilton, and in regard to himself manifests the indomitable spirit which, carrying on a constant battle with ill-health, was one of the many charms of his noble character. I here give this letter, and add a sequel of shortly subsequent date and equally characteristic.

From PROFESSOR SEDGWICK, F.R.S., *to* SIR W. R. HAMILTON.

'CAMBRIDGE, *March* 26, 1854.

'How long your *Lectures on Quaternions* has been at my rooms is more than I can tell you: but here it is before me, with "the author's affectionate regards" written on the title-page. I am very oblivious; so I may have thanked you before; but I have no remembrance of having sent you a former letter of thanks, and I now do thank you with my whole heart. Your book is quite out of my depth, and I am far too old, and too busy in the works of my own craft, to think of learning to swim in the wide ocean of your mathematical speculations; but I can, and do, wish you good speed, and I trust that your life, spent so nobly in soaring after truth, may be continued to you in honour and happiness for many years to come. I have had a miserable winter. In the October Term I lectured (my thirty-sixth Course) to the largest class I ever had at Cambridge; and I always had a good class, even during the twelve or fourteen years when the professorial lectures were, generally, in a state of great depression. *Now* they are much revived. Perhaps the crowded state of my lecture-room excited me too much; or perhaps I this year had a warning that I am becoming old, and unfit for my former tasks. Be this as it may, I did break down during the middle of my course; and I was forced for fifteen days to be as mute as St. Anthony's congregation in the Gulf of Genoa (surely it was there or thereabouts that he preached to the fishes; but you can, I dare say, eliminate the fact without the help of quaternions). During the said fifteen days my inner man was washed by streams of rattling cathartics; and as for my poor head, they cured its congestion by sucking all the blood out of it by help of leeches applied three different times to my temples, the back of my ears, and the back of my neck. I then finished my course, though in a languid and unsatisfactory manner. The general reduction of my system made me unusually sensitive to atmospheric impressions, and before the end of Term I caught a cold which assumed, in a day or two, the type of bronchitis. For twenty-nine days I remained a prisoner in my own room, and in a state of miserable stupefaction. The weather then became milder, and about the middle of January (muffled to the ears, and with a *respirator* over my mouth and nose) I went to my house at

Norwich; and under the care and love of a niece* I gradually came round, and again tried to spread my sails to the wind when the weather was mild and the sun was warm. I returned to Cambridge early in this month. I am still a grumbler. A remnant of bronchitis still clings to my chest; suppressed gout takes away my sleep; my kidneys are doing their work in a grating fashion; and I have every day, especially every night, long fits of coal-black melancholy. A quaternion of maladies! Do send me some formula by help of which I may so doctor them that they may all become imaginary quantities or positively equal to nothing. Any change will be for the better. Try some transformation or other; for I am at present both stupified and demoralized. I send you this dissection of myself as a kind of lame apology for not having thanked you sooner. Your book was, I believe, on my table among a multitude of parcels I found there, about three weeks since, when I returned from Norwich. Give my best remembrances to Professor Lloyd and the other members of the dear late Provost's family. Ever, my dear Sir William, truly and gratefully yours.

'P. S.—I may well talk of getting old, for on the 22nd of this month I entered on my 70th year, and I have now resided very nearly half a century in Cambridge. I was a green Freshman in 1804.'

From the SAME *to the* SAME.

'CAMBRIDGE, *April* 19, 1854.

'Verses I have none to send you, for I never wrote a line of poetry in my life—at least in my own Yorkshire tongue. My master, under the stimulus of birch, did force out of me some Latin lines that would scan; but I dare say he never had a drop of poetry from me. No muse ever rocked my cradle: but I love good poetry and good poets, and I do fervently thank you for your last letter and the enclosed poem. To offer you one of my geological Papers would be like asking you to lunch on a dish of brick dust. I have, however, found a copy of my letters to Wordsworth, which are a little more popular than my Paper read at Somerset House. I am not so unreasonable as to ask you to read them. I only ask you to

* Miss Isabella Sedgwick.

read from page 51 . . . 53, and the concluding words beginning page 90.

'Pray accept this small and humble offering as a mark of my regard and my affectionate remembrance of your kindness.

'Ever, my dear Sir William, truly and gratefully yours.'

Sedgwick's 'charming letter,' as Hamilton called it, was forwarded, on account of the message at its close, to Professor Lloyd, and with it a note of De Morgan's, in his correspondence with whom he had found occasion to criticise some assertions of Auguste Comte's in connexion with mathematics. What passed between Hamilton and De Morgan on the subject may be found in its place at the end of this volume, but I insert here Hamilton's reference to it in his note to Lloyd.

From Sir W. R. Hamilton *to* Professor Lloyd, d. d.

'*March* 31, 1854.

'. . . De Morgan I have, I think, not *seen* oftener than once, or say twice, in my life, but the triplets and quaternions threw us into a very free and familiar correspondence; and I spoke to him of Comte lately with a degree of freedom, which might well have been called *flippant*, if my criticism on one part of the writings of so celebrated a man had been in any way designed for publicity. *Some* of the mistakes may be mere slips of the pen, but others (I still conceive) indicate a confusion of thought on Comte's part, on subjects which he professes to treat of.'

In May Hamilton wrote an important letter to John T. Graves, in which he comments on Mr. Graves's theory of Octaves (a theory arrived at independently by Mr. Cayley), and proceeds to announce that 'one of my recent extensions of Quaternions gives a theory of *quadrinomes*,' and, 'I have found also a system of Tetrads.' A postscript refers in appreciative terms to the researches of the Rev. T. P. Kirkman, and on this account I insert it in a note, leaving the main body of the letter for a place in

Hamilton's scientific correspondence.* The same must be the destination of some still more important letters to Dr. Lloyd. They set forth what he calls a conjecture suggested by Quaternions which might prove 'a physical discovery respecting the mutual action of two elements of the same, or of two different (electro-magnetic) currents, considered as exerting (in addition to Ampère's *attractive* or repulsive force) a certain *directive* force, or as producing a system of *two contrary couples*.' He afterwards saw reason to doubt of the physical applicability of what he had called provisionally his electro-magnetic Quaternion; but Lloyd continued to assign to it a high possible value in relation to the theory of Electro-magnetism. Dr. Lloyd's words are—

'*May* 31*st*.—I am greatly interested with your electro-dynamic Quaternion. It seems to me to promise (*not* a new physical discovery, but what is yet more interesting) a theoretical explanation of the fundamental facts of electro-magnetism. . . . The similarity (or agreement) to these [Biot's laws, representing the action of an *infinitely small magnet* upon *a magnetic particle*] of the laws which govern your *vectors*, give, I think, ground for hope that you will be able, through it, to explain the true physical relation between the electric current and the magnet. And if so, the discovery will indeed be a great one.'

I add here a postscript to one of these letters to Lloyd, indicating the fertility at this time of Hamilton's mathematical imagination :—

* '*May* 16, 1854.—I have little more than looked into Kirkman's Pluquaternions (*Philosophical Magazine*, end of 1848), but am sure that they are very interesting, and ought to be studied, if the octaves are to be pursued. You know I object to his use of the word *bi*quaternions, which I wish to reserve for $Q + \sqrt{-1}\, Q'$, Q and Q' being quaternions. Did you ever consider whether he has been sufficiently cautious in his use of the association principle? And must his curious "imaginary duads" finally remain as such? Are his final conclusions about sums of squares confirmed? He is a very clever fellow. My son William Edwin has lately received another science honour in T. C. D. Salmon's *Conic Sections* he knows a good deal about, and is now entering seriously on the Differential Calculus, of which I must soon tell you *my* definition.'—See *Elements of Quaternions*, pp. 99, 100.

'*May* 30.—P. S. This month of May has been an unusually original (if not an unusually busy) one with me, whatever good may or not have come out of this last speculation. At least four distinct (though not unconnected) trains of inquiry have opened themselves, though I may not be able to pursue any of them far at present; and I could not continue the writing of a letter to Carmichael* this morning, which had been begun yesterday evening without perceiving a fifth line of future investigation. All this, however, is unfavourable to routine duty: and, besides, we are at present in much anxiety here about the health of my dear little daughter.'

A cheer was given to Hamilton's spirits this summer by two events: in July the living of Loughcrew was, through his influence, conferred by the Lord Lieutenant, Earl De Grey, on the Rev. James Alexander Hamilton, son of the uncle to whom he had been so deeply indebted for his education; and in August the Board of Trinity College relieved him of all remaining liability for the expense of printing the *Lectures on Quaternions*. They had previously contributed £200 towards this object, but a balance of £100 remained due, and the prospect of having to pay this sum was the source to Hamilton of much anxiety. His cause was warmly advocated by Dr. Luby, Dr. Todd, and Dr. Lloyd, and the vote in favour of the grant was unanimous. Dr. Luby had previously written to Hamilton, saying: 'I cannot conceive that the Board would suffer you to be at any loss by the publication of a work which is so noble a monument of human science, and which does so much honour to our University.' A pleasant note from Dr. Lloyd, of subsequent date, makes the following reference to what passed on the occasion. 'I have often

* The Rev. Robert Carmichael, mentioned here, was a Fellow of Trinity College, who carried on original researches in the region of Quaternions, and corresponded with Hamilton on the subject. One formula $(D_x - iD_x - jD_y)V = 0$, connected with the celebrated equation of Laplace, Hamilton proposed to call 'Carmichael's Equation': see *Proceedings*, R. I. A. Hamilton somewhere notes, concerning Carmichael, that 'he often made mistakes, but that he had the temperament of genius.' His life was cut short at the early age of 31.

thought since I saw you with equal pleasure and pride of the *Quaternion* you told me of, whose unit-vectors are (not *i*, *j*, *k*, but) H., J., L.;* and I made some use of it at the Board the other day *in solving the problem of Gill's account!* I must add, however, that the members did not need much *demonstration* to arrive at the conclusion which they came to.'

Dr. Lloyd at this time lived in a country house called Kilcroney, situated near the entrance of the Dargle, and here he and Mrs. Lloyd hospitably received many distinguished persons, foreign and native, whom the eminent qualities and accomplishments of the host and hostess, and the rare loveliness of the surrounding woods, and waters, and mountains, attracted as visitors. Towards the close of August, Hamilton took the thought of going to see these faithful friends, found them at home, and was easily persuaded to stay the night.

In the following Hexameters he puts on record the incidents of his visit. They were sent to Mr. Thomas Disney, who, by lines of his own in the same metre, had provoked Hamilton's attempt to repay his friend in kind. It cannot be said that they cope successfully with the difficulties of the metre. His earlier poem on the Dargle, to which he refers in the letter prefixed to some copies of the latter one, printed for private circulation, may be found in the first volume of this work, page 147. I reprint, with the Hexameters, the prefatory letter;—

From Sir W. R. Hamilton *to* Thomas Disney, Esq.

'Observatory, *September* 15, 1854.

'You will easily believe that in allowing the following verses, connected chiefly with the scenery of the Dargle, to be printed, though not published, my highest ambition has been to give some moments' pleasure to a few friends and acquaintances, for whom I had at first designed to copy the lines myself, but found that the task would have occupied too much time. If those verses shall fall into the hands of any persons unacquainted with me, I hope

* Hamilton, Jacobi, Lagrange; *supra*, p. 117.

that on inspection they will acquit me of the presumption of having aspired to *rival* any compositions in the same unusual and somewhat foreign metre, which yet, I may be supposed to have sought for my own satisfaction to *imitate*.

'You know that I have never hoped to win any reward from poetry, beyond an occasionally elevating relaxation from pursuits of a different kind, and an assistance towards preserving for myself, and sometimes communicating to friends, a few records of the pleasurable or painful feelings of what has been, upon the whole, a studious and happy life. My recent attempt at hexameter composition, notwithstanding its confessedly imperfect execution, has already enabled me to enjoy more than before (you will say, perhaps, by force of contrast) some passages in the "Evangeline" of the American poet Longfellow, not to mention here any less popularly known writers in that style. But I am so far from supposing that I have surmounted the (by some thought insuperable) difficulties of this kind of verse in our language, that these latest lines of mine do not please myself so well as even that early, and indeed boyish poem of my own, on a subject nearly the same, which I venture here to append: although, as a record of the sort above referred to, I have chosen in this way to preserve them. You will see that I have abstained from introducing the name of any living friend; but have thought myself permitted to allude to a dear and departed lady, whose name has become already classical, from its occurrence in the published Memoirs of her deceased and illustrious father.'

'DARGLE VERSES.

'TO AN OLD FRIEND.

'Thanks, old friend, for your lines, in that ancient hexameter flowing:
Thanks for describing so well to me the adventures of that night,
And whatsoever befell, since alone in the starlight we parted.
I have been roaming too, and have met with some pleasant adventures;
Such will I now recount: accept the imperfect requital.

'After a morning walk to the nearest skirt of the city,
Back by the cool canal, and the long green lane to my dwelling,
Musing I came; and enjoyed the distant vision of Dublin,
Seen with the valley between, and the ship-studded ocean beyond it,
Spires, and the haunts of men, while I stood in unbroken seclusion,
Near that small gate which you know, where the harvest waved, ripe for the reaper.

'Then to myself I said: I'll enjoy the remainder of this day;
Let books rest for awhile, and the still new labour of writing:
I will return to men, yet see more of nature this evening.
Lately I have not viewed so much as the borders of Wicklow.
There, where that hill-top appears, from the seaside rising abruptly,
Though from my sight its base by nearer ranges be hidden,
Lovely the scenery is, and to me was in boyhood familiar.
And with a dweller there, from early youth I'm acquainted:
Known o'er the world his name, with our Irish science associate,
Yet by his friends beloved, e'en more than admired by the stranger.
Worthy his wife to be his, but of her let these verses say only,
Her's is the name of *your* wife, and that of the Daughter of Wordsworth.
Yes, I will visit them, and enjoy the scenery near them.

'Easily was I persuaded, when this my resolve was accomplished,
And when at evening hour I stood in the friendly mansion,
There to remain for the night, and explore on the morrow the Dargle.
He, then, and I set forth, on that our pleasant excursion.
Leading a child by the hand, for whom at the font we had answered,
To us his brother came up, who was one of my old College cronies.*
Stoutly we battled it out, our collegiate arena when entering;
Till we from rivals grew friends, and such have we ever continued.

'So we together went on, and remembered beauty received us;
Which, when an ardent boy, I had sought to describe in my verses:
But had despaired, and now, much more, must despair to describe it.
Yet, though the air was warm, I longed for a perfecter sunlight,
Some old effects, which I missed, to bring out on the canvas of nature.
Nor was the wish confessed by me, at last disappointed:
Burst forth the sun in heaven; wood and water glittered in sunshine.

'Leaving the lovely glen, the prospect expanded around us;
But it was lovely still, and only succession of beauty.
Not in my friendly guide had the sense of such pleasures been blunted,
Nor our home charms despised, from his knowledge of men and of cities.
Venice he could describe, and its first effect on a stranger;
Switzerland had he searched, and many a mountain ascended:
Yet he enjoyed, like me, the view that was open before us.
As on green slope we lay, we touched upon manifold topics;
Some that referred in part to new speculations of science,
None pursuing so far, as to trouble our tranquil enjoyment:—
Talked of the planet-spheres, and are they peopled by beings,
Gifted, like us, with mind, and, ah! like us, loving, but sinful?—
And, while the sheep on the hill, at our feet, were quietly grazing,
Spoke of the BOOK DIVINE, so rich in pastoral image,
Lambs, and the heavy with young, and the tender care of the SHEPHERD.

* Bartholomew Lloyd, Q. C.

'Then we descended to cross to the other side of the valley.
Boat was there none, us to ferry the murmuring rivulet over:
Deftly we picked our steps, on the small rocks, moistened and slippery.
And I remembered the Wharf, near Bolton Priory gliding;
Broadly and stilly it flowed, by a long line of stones intersected;
Pleasant passage which gave to me last autumn at sunset,
Wishing, from hill that crowned the further bank of the river,
On the gray Abbey to gaze, and to muse on the White Doe of Rylstone;
Ere in my wanderings I reached that Strid, so fatal and famous,
Where the pent water boils, and the Boy of Egremond perished.—
Parting at length from my friends, I set out on my homeward journey:
Here in the twilight arrived, and found your epistle awaiting.

'Thus have I, mingling thoughts of old times with recent adventures,
Simple however they were, and involving nothing of wonder,
Told you, in verses rude, and metre to me unfamiliar,
Story of those two days, which have passed since we last met together.

'*August 26th*, 1854.'

The British Association met this year in Liverpool, in the month of September, and Hamilton attended it, principally with a view of securing that it should make its second visit to Dublin in the year 1857. This he succeeded in arranging. I find, cut out from the *Morning Post* of September 30, the following report of a speech made by him when proposing a vote of thanks to the foreigners who had attended the meeting. Its allusions to the Abbé Moigno and Monsieur Foucault give it a personal interest, and the references to poetry, and the poetic treatment of his subject, are characteristic of the speaker.

'My Lord Harrowby, and Ladies and Gentlemen,—

'I have been requested to move, that the thanks of this Association be given to our distinguished foreign visitors. In the first place, allow me to thank you for having thought that I might, without impropriety, be put forward on such an occasion, and for having shown thereby that you do not consider me as a foreigner. Since I was invited to make this motion, I have had no time even to read the list of our foreign visitors, and must request them and you to acquit me of the presumption of pretending to analyse that list, or of doing anything more than state my own personal views. Of the members from America who may be present I shall

say nothing, for in England and Ireland, and I suppose especially in this great port of Liverpool, we scarcely count Americans as foreigners. From the scientific and philosophical writings of the Germans, I hope that I have learned much in my maturer years; but when I was a boy the French were my masters and teachers in science. In Ireland, at that time, the higher branches of analysis went commonly by the name of French mathematics; and, in order to study the works of the great writers in that department, I was obliged to learn their language. Having never yet enjoyed the advantage of a visit to Paris, though I have had the honour of being, for the last ten years, a Corresponding Member of the Institute of France, the scientific names of that country have appeared to me pretty much as abstract ideas; but I think that when you look upon the eminent Frenchman at my left hand you will agree with me in considering that he is no mere abstract idea, but a very concrete, and a very pleasant body. (This allusion to the fat little Abbé Moigno produced great laughter at the moment.) Perhaps you may have thought, any of you who may have happened to observe me turning over the pages of a book upon this platform, within the last half-hour, that I have been inattentive, negligent, and almost rude, as regarded the proceedings of this meeting. But when I tell you that it is a work put recently into my hands, by my present neighbour, the Abbé Moigno, whom I had known to be an abstract mathematician, but who now turns out to be a distinguished physicist also; and when I add that in this work ("Répertoire d'Optique"), I find expressed the most candid, intelligent, and, I must say, generous appreciation of what has been done by persons in these countries, you will forgive me that seeming neglect. As an old member of this Association, it has gratified me to see noticed in this Optical work of a Frenchman an account of experiments in Optics, which first acquired some publicity during the first Cambridge meeting of this Association, in the year 1833. And if, with the name of my dear Irish friend "Monsieur Lloyd," the name of "Monsieur Hamilton" be coupled, I can scarcely be expected to feel less grateful on that account. I can remember the time when England was at war with France. I was a child then, but children quickly learn, through sympathy, the feelings of their seniors. And I remember that France was then, I do not say feared, but respected, as a formidable and gallant

enemy. The French were then our generous and noble foes; they are now our generous and noble friends. I trust—indeed I feel assured—that in making such allusions as these to events which now agitate the world, I do not pass beyond the limits within which the rules of the Association confine its members. The names of Whig and Tory are never heard within our halls; those party differences which are, perhaps, essential to the working of a free State, find yet no echo here. Some may, perhaps, expect that we should carry out this principle yet further, and that we should realise in our own persons the conception of the Roman lyrist:—

> "Si fractus illabatur orbis,
> Impavidum ferient ruinæ."

And our great country *has* stood firm, while thrones were shaken, and nations were reeling around it. But individuals may be excused if they exhibit a less absolute and philosophic calm, and if they suffer to be perceived an ardent, and even an anxious interest, in the fortunes of the land to which they belong, especially in this Society, which, if it be an association for the advancement of science, is yet essentially a British Association. Permit me, for a moment, to make one other allusion to war, and to name another Frenchman. When Professor William Thomson, of Glasgow, was lately speaking in our Mathematical Section of potential and actual energy, and drawing one of his many and happy illustrations from the gunpowder and the cannon ball, I was reminded of some lines of Coleridge's translation of Schiller's play of "Wallenstein." In that great play, and in that beautiful translation, the elder Piccolomini (Octavio) is represented, if my memory serves me aright, as warning against the hazards of civil convulsion that son of his, Max Piccolomini, on whose character the author of the poem appears to have lavished all his love, and to have bathed it with floods of lustre. The father, I think, is represented by Schiller as saying to the son, respecting the ball when it has left the cannon—

> "It is no longer a dead instrument;
> It lives, a spirit passes into it."

And I must say that when we in general, and more particularly when a small party of us, of whom I was one, were permitted lately by M. Foucault to attend the exhibitions of his more recent

experiments, the same lines of English translation of German poetry again occurred to my remembrance. When I saw the little disc of Professor Foucault possessed with what he was pleased to call "*un esprit de contradiction*," disobedient, apparently, to its human former but yet essentially and throughout submissive to the laws which the great Creator of all things has been pleased to impress upon matter, I admired anew the activity of the French intellect; but I looked up with even greater reverence than before to the Supreme Giver of all intellectual and of all higher treasures.'

From Liverpool, with his son Archibald as his companion, he proceeded to the Lake Country on a visit to myself and Mrs. Graves, then residing at Dovenest on Windermere. During this visit he had the satisfaction of being several times received affectionately at Rydal Mount by the widow of Wordsworth; he enjoyed at Lancrigg the hospitality of Mrs. Fletcher and her daughter, Lady Richardson; and he renewed a long discontinued intercourse with his old friends Mr. and Mrs. Richard Napier,* then sojourning near Rydal.

A letter, written in a somewhat playful spirit, to his old friend Lady Campbell, discovers in Hamilton the elements which were deeply seated in him of a reverent admiration for woman showing itself in the form of a grave, old-fashioned, gallantry. I have in a former volume† put on record how eminently fitted was Lady Campbell to call forth these feelings. Not a less noble example of womanhood was Mrs. Fletcher of Lancrigg, the principal theme of this letter to Lady Campbell. No one who had ever met her could fail to carry away the impression that he had met in her a woman almost unique for the combination of majestic form and features with a spirit and an intellect in perfect correspondence. Enthusiastic and yet dignified, strong in will and judgment, warm and compassionate in feeling, she was the ideal of womanly grandeur—a queen of nature's making—receiving, not claiming, universal homage. But I need not to set out in detail her eminent qualities

* *Supra*, vol. i., p. 155. † *Supra*, vol. i., p. 359.

or the influence she exerted over her friends. That has been done by the publication, since her death, of one of the most delightful of autobiographies, enriched by the reminiscences of Lady Richardson, its editor, and of her sister Mrs. Davy, daughters worthy of their mother. The act of homage to his venerable hostess which Hamilton records had therefore, it may be seen, the warrant of a special fitness.

From Sir W. R. Hamilton *to* Lady Campbell.

'House of the Rev. Robert Graves,
'Dovenest, near Windermere,
'*Friday night, October* 6, 1854.

'Will you believe that I sat next a gentleman here, at dinner yesterday, who asked me if I had lately seen "Pamela Campbell"? a name by which you know that I never presumed to call you. You will think that the man must have been very impertinent: but will, I suppose, forgive him and me, when I tell you that he was Mr. Richard Napier, brother to the conqueror of Scinde, who is, I believe, a cousin of yours, and who is pleased to claim an acquaintance with me of thirty years' standing. Again, at dinner to-day, but at another house, your name was mentioned by a lady —and a very old and very charming one—who told me that she remembered Sir Guy Campbell since he was "that height" (about the height of the table), and that she knew he had married a daughter of Lord Edward Fitzgerald. I said that another Guy, son of her friend, had died last year in India, and that his mother had scarcely yet recovered from the grief occasioned by the loss. She inquired whether you had any other children, and I mentioned Sir Edward and your daughters. She wanted then to know whether they had inherited the beauty of their parents. I said that I had always heard that the married daughters, of whom alone I should presume to speak, had been quite "the rage," for their beauty, &c., at the Irish Court, and elsewhere. It is time that I should tell you at least the name of the lady who thus catechised me. She is Mrs. Fletcher of Lancrigg, and she is in her 85th year. She took my arm, when we were going in to dinner, and her conversation was really delightful. She remembers well and vividly many of the remarkable characters of the latter part of the last

century—especially those connected with Scotland, and, still more precisely, with Edinburgh, where she at that time resided—for example, Dugald Stewart. She has been well acquainted with Wordsworth during a long subsequent residence in this neighbourhood; and she, and her daughters (one of whom is lady Richardson, wife of the Arctic traveller), have also deeply appreciated him. (I have visited alone, by moonlight, the graves in Grasmere churchyard, of Wordsworth and his daughter Dora, and others of his family whom I remember. I have also visited his widow, who received me several times affectionately at Rydal Mount). And yet she (Mrs. Fletcher) is quite open to fresh impressions—for instance, to the merits of Matthew Arnold's poetry. By-the-way, Mr. Arnold and his wife were members of our dinner-party. To sum up all, she (Mrs. Fletcher) has *almost* the finest eyes in the world, and when retiring in the evening, I asked leave to kiss her hand—an action which I found was considered to be quite *comme il faut*, and in which my son Archy followed me.

'I write in the bedroom which once was occupied by Mrs. Hemans, and the view from which is lovely. It is nearly time for me to go to bed, especially as I have walked several miles to-day. Good night, dear Lady Campbell, and believe me to remain your affectionate friend.'

He also met at Dovenest a remarkable man, William Pearson of Borderside, Crosthwaite, and his estimable wife, born Greenhow. William Pearson was a self-cultured yeoman, who at this time, after many years spent in a Bank at Manchester, had retired to a small patrimonial estate on the southern border of Westmoreland. Here a stranger might have been surprised to find him surrounded by a choice collection of books representing fully the English poets of all ages, and (in translation) the best German authors, poetic and metaphysical, besides the classical prose works of his country and the best authorities in Natural History. Of the habits of birds and other native creatures around him he was a watchful observer, and he described them in purest English, with a charm that suggested no disadvantageous comparison with White of Selborne. Soon after his marriage, which occurred late in life, he spent a year in Switzerland, and Wordsworth, who had long

known and esteemed him, told me that the brief records made by him of noteworthy incidents of travel or natural phenomena were models of what a tourist, eschewing the commonplace and the superfluous, should indite. Wordsworth's poems he particularly loved for their truth in the expression of the contemplative mind occupied with outward nature and its own higher aspirations; he knew most of them by heart, and his spontaneous recitation of them was scarcely inferior in feeling and truthfulness to that of the poet himself.

I should say more of him and of Mrs. Pearson, had not the latter, by a biography,* printed for the enjoyment of his friends, preserved from oblivion the character and some of the writings of her husband. I remember that at Dovenest the Yarrow poems of Wordsworth, illustrated by all the ballads which prompted them, afforded delightful subject of discussion to him and Hamilton, and to others taking in it a subordinate share, and that from poetry the keen intellect of the rural philosopher passed on to metaphysics, and urged the scientific sage, not without some reward and satisfaction, for explanations and judgments. The question, then much canvassed, of the plurality of inhabited worlds excited between them a lively difference of opinion; Hamilton, though refraining from a negative conclusion, agreeing with Whewell in considering that science afforded no analogy in favour of the idea.

Hamilton and his boy accompanied Mr. and Mrs. Richard Napier across Dunmail Raise to Keswick, and proceeded by Whitehaven to Belfast and Carlingford, where he was again the guest of Mr. and Mrs. Disney. In the middle of October he was at home in his Observatory, where the only exciting event of the closing year was a visit from the deaf and dumb boys of Claremont Institution, to whom Hamilton and his assistant showed the telescopes and some of the heavenly bodies, receiving afterwards

* *Papers, Letters, and Journals of William Pearson:* London: Emily Faithful, 1863.

in return what he calls 'a most delightful and characteristic letter of thanks.' In simple terms it expressed admiration of the proofs of God's Almighty power, and gratitude for the bread, jam, coffee, and milk supplied to them by Lady Hamilton. De Morgan commented in his witty way upon this letter, writing about it, if I remember rightly—'These D.D.s are like other D.D.s in wisely linking together the good things of heaven and earth.'

Readers who remember the terms of friendship which in early years subsisted between Hamilton and Lady Campbell will have noted with pleasure that he continued to correspond with her, and will be glad to learn that he still enjoyed the advantage of being able to confide to her his inward trials, and to speak to her freely of his spiritual state, receiving in return kindly sympathy and wise counsel. As a true friend, she had not failed earnestly to plead with him his own cause, when she heard of the danger that threatened him of being dominated by a fatal habit; and now, at the beginning of this year, congratulating him on the publication of his *Lectures*, she had written—'I hear the Quaternions is a wonderful book, and sheds a light on Ireland. You know how I love to be proud of you, so I need not tell you how I rejoiced to hear this.' In Hamilton's letters of this period to his old friend, the portions which are most interesting are such as it would not be right to give to the public, and those of slighter texture would not well stand alone. I must content myself with producing notes referring to small books published by Lady Campbell, which were marked by a beautiful delicacy of touch in description and by justness of feeling, and with extracting one passage from a later letter which incidentally presents a combined retrospect of the external and spiritual life of the writer.

'OBSERVATORY, *May* 17, 1854.

'So while you were seeking to draw me out, on May-day last, to talk too much about what you were pleased to call my "wonderful" book, you did not give me the least hint that there was a chance

of my so soon seeing your "charming" one . . . Now I won't begin to praise you even as an author, and can't say that I find that anyone here is disposed to *wonder* at your producing a delightful book. But I may mention or confess that, living so much out of the world as I do, it was not till yesterday that I heard of the publication of your *Cabin by the Wayside*, and forthwith pounced upon it at M'Glashan's . . .

But your book fairly carried the day, or the evening, as I was walking back along the Canal yesterday, after having walked to Dublin some hours earlier by the same path. A couple of milestones gave me very pleasant opportunities for sitting down to read more comfortably, and when I got home I was so selfish as to finish the book quite alone. But this morning I showed it to Lady Hamilton. . . . At all events she never stopped till she had read aloud the whole to my little daughter, and to a son, who pretended to be reading Homer. I have put my daughter's name in the copy.'

'*May 18th.*—Unquestionably I shall accept with pleasure, not to say with pride, the copy which you promised me of *The Story of an Apple;* and what's more, that I may have the pleasure of reading it first, as a gift from you, I shall abstain from ordering the book. My son Archy has confessed that he was listening while his mother was reading your book aloud.'

'*June 15th.*—It was Jellett's turn, as one of the Examiners for Bishop Law's Mathematical Premiums, to give the annual dinner. I sat between him and Mr. Walsh, a clever barrister,* and remarked to the latter that this was the 28th time of my assisting at an Examination Dinner of this particular kind. "What a frightful restrospect!" exclaimed he. "Do you mean the dinners?" I asked him. "No," said he, "but the years." I let the conversation on that subject drop; but cannot altogether regret that I have lived so long, though I have had impatient fits. Or rather, in my serious moods, I feel it to be a subject of deep thankfulness that so much time has been allowed for *schooling*—a process which as yet has been very imperfectly performed in my own case . . . if I find and transcribe them [some notes written at

* Afterwards Master of the Rolls.

the commencement of the preceding year], you will see that, when thoughtfully considering the question, I have heretofore recognised, as I still do recognise, a *kindness* in life being spared. . . .

P. S.—I have played a few games of draughts this morning with my daughter—dear, sweet, patient child. She has very few sources of amusement, but she enjoys flowers, the garden, and the Bible.'

CHAPTER XL.

CORRESPONDENCE WITH AUBREY DE VERE: WITH THE NICHOLS: CIPHER: MRS. WILDE.

(1855.)

THE year 1855 was not remarkable in Hamilton's history, either for scientific work or for events of importance; yet there remains of it extensive record in the form of correspondence. A summary of the facts, followed by some extracts from the correspondence, may here suffice.

The correspondence between Hamilton and Aubrey De Vere springs again into activity. It had been interrupted by an illness of the latter from scarlatina which precluded for a time intercourse of any kind. In its course—it went on continuously into the succeeding year—Mr. De Vere wrote some most interesting letters, giving an account of his gradual change in religious opinion, until his final submission to the Roman Catholic Church. Hamilton declines to discuss the great question controversially, but gives his friend clearly to understand that his own opinions, instead of moving in the same direction, had become more Protestant. Had Hamilton's letters met those of his friend by argument, I should have thought it a duty to print both sides of the correspondence in full; but besides the limits of space which I am obliged to observe, I have to consider that these letters of Mr. De Vere belong more properly to the history of his own life. It is to be desired, I think, that they may see the light, for undoubtedly they deserve meditation and discussion, as worthily expressing the views which proved decisive with a singularly pure and thoughtful mind. I say this, though myself farther removed, perhaps, from his stand-point than was

Hamilton, of whom it is only fair to add that, writing subsequently to another friend, he calls Aubrey De Vere a 'splendid type of Romanism.' It is true also that, although the two friends diverged widely as to the Church question, they continued to the end to be in harmony in respect to their views of religious philosophy; Hamilton, as well as De Vere, maintaining that, from the constitution of the human mind, Faith had in Religion a function at least co-ordinate with that of Knowledge.

The meeting of the British Association attracted Hamilton, in the September of this year, to Glasgow, and there, at the Observatory of the University, he became the guest of his friend Dr. John Pringle Nichol; and an intimacy thus arose between him and the family of the accomplished astronomer, which continued to be highly prized on both sides. Dr. Nichol's son, now Professor of English Literature in his father's University, was then a member of Balliol College, Oxford, and already manifested that taste for modern literature, of which he has since given to the public proofs which mark him out as not only a vigorous critic, but a man of poetic power. The confidence which his father had won from Hamilton was now, with full expansion of feeling, imparted to the son; and the daughter and sister of these men, Agnes Nichol (afterwards to become wife of Dr. Jack, Professor of Mathematics), herself imbued in literature, and exercising in it her inventive faculty, was admitted a member of the band of friends. The correspondence which resulted was frequent and animated; but as on Hamilton's side it was mostly occupied with the incidents of the past, a region which the reader has already traversed, I refrain from quoting it, except in brief extract.* One

* It was about this time that John Nichol, Jun., addressed to Hamilton two sonnets, which tended to cement the friendship between them. These sonnets dwelt upon the combination in Hamilton of simplicity and affection with high scientific qualities; but some imperfections of expression rendered their author dissatisfied with them, and he afterwards condensed their meaning into a single sonnet, which has appeared in the collection of poems recently published by him. A similar tribute to the memory of his father may be found in the same volume, and in this connexion will interest the reader.

subject of Hamilton's letters to Dr. Nichol and his son was a cipher of his own invention. Some of the extracts which follow will inform the reader of the great merits of this cipher, the almost unlimited power of variation which it admits of, and the practical impossibility of discovering its key. Hamilton was wont to say that he believed a challenge to the whole world to discover it would not meet a reply.

The communication which Hamilton made to the Mathematical Section of the British Association at Glasgow was a Paper *On the Conception of the Anharmonic Quaternion and on its Application to a Theory of Involution in Space.* During the week of the meeting he joined an excursion-party to the Isle of Arran, and with a select number of its members was received by the Duke of Hamilton at a *déjeuner* at Brodick Castle, Sir Roderick Murchison introducing him personally, and bespeaking for him the place of honour among the guests. This visit was one of great enjoyment to Hamilton. The conversation which passed led to a short but cordial correspondence. Hamilton had mentioned, perhaps quoted, to his host Aubrey De Vere's poem to the memory of the *Highland Mary* of Burns, and the Duke had the tact to recognise in Hamilton a namesake as well as a savant. Accordingly Hamilton afterwards forwarded to him a copy of Mr. De Vere's poem, adding (let it be confessed) his own *Dargle Verses*, and made good his title to his name by using and calling attention to the impression on the envelope of his father's seal, which bore the Hamilton crest and motto; and in reference to his own branch of the family, in a subsequent letter to the Duke, he took occasion to write:—'Sir W. R. H. has, with the exception of his two sons William and Archibald, only *one* living male relative of his own name with any traceable degree of connexion: namely, his cousin the Rev. James Hamilton, who has the living of Loughcrew in Meath, and who (according to family tradition) is the heir to a dormant baronetcy.'

On the day he left the Observatory of Glasgow Hamilton composed a sonnet prompted by a photograph of his daughter, which

he had received as a gift from her on the eve of his departure from home. This sweet and interesting child had wound all her tendrils round her father's heart. In one of his letters I find him writing:—"I think Lady Hamilton would forgive me for saying that the being I now love most in the world is my little daughter;" and elsewhere he declares that he is in the habit of judging the happiness of other men by considering whether they have or have not a daughter. It may be added, that even at this time—she was now only fifteen—she had evinced much love for poetry, with which she had largely stored her memory, and that having been brought through more than one serious illness, she had been very recently a sufferer from an accidental burn of considerable severity.

'SONNET TO MY DAUGHTER.

'Dear patient child! upon a bed of pain
So lately lying, watched by tender eyes,
Thy sun-limned face and form I dearly prize,
Thy gift at parting; and can see again
Thy head bowed meekly o'er some poet-strain,
In book out-spread, or some diviner page,
Such as would oft thy maiden thoughts engage
Ere yet I left thee for the ocean-plain.
Not all the wealth of mind, not social joy,
When Scottish Science met in converse free
With men of other lands, and welcomed me,
Away from thee could all my heart employ.
In starry tower, or on the sunny water,
I blessed my loving and beloved daughter.

'OBSERVATORY, GLASGOW, *September* 24, 1855.'

Late in the autumn of this year a visit was paid to his own Observatory, which stirred into activity remembrances which had not ceased to be cherished by Hamilton, and enabled him to show the interest of a friend in persons whom he had not before seen. One day in October, Mr. Robert O'Brien arrived at Dunsink with his two young daughters and their two brothers. Mrs. O'Brien, formerly Ellen De Vere, had desired that her husband and children should be known to Hamilton and his wife, and

advantage was taken of their being for a short time in Dublin to make an arrangement for the purpose. Friendly feeling was manifested on all sides, and Hamilton took special delight in showing to the daughters an experiment which he remembered having in the same meridian room shown to the mother, and never in the long interval to anyone else—that of a key floating on mercury—and he was prompted, when the visit was over, to compose a sonnet addressed to the two young girls, which in affectionate terms expressed the pleased and tender feelings with which he had welcomed them to his house, and joined them to the company of friends, living and dead, whom he liked to associate with the thoughts of his garden :—

'Mary and Alice! though before that day
Unmet, when, through this garden wandering,
Lately ye trod where Wordsworth once deigned sing
Some grand old strain for me, or newer lay
Murmured, half musing, or in boyhood's May
With Brinkley spent I sweet but solemn hours,
Listening his lovely lore of stars or flowers,
The thought of you not soon shall pass away!
Your images shall rise before me here,
With thoughts of other friends of long ago:
Time takes not all things in his ceaseless flow,
Fixes and hallows some: and many a year,
Since dear ones linked with you I first did know,
Gives me a poet's right to call you dear.

'Observatory, *November 5th*, 1855.'

In the spring of the year Hamilton met, for the first time, Mrs. Wilde, afterwards Lady Wilde, the wife of Sir William R. Wilde, the eminent physician and Irish archæologist, herself remarkable as a woman of warm feelings and literary faculty, shown in patriotic and eloquent contributions to newspapers and other less ephemeral periodicals, and in poems published under the name of *Speranza*, and, it may be truly added, as a woman of high aspirations and real genius. The acquaintance rapidly ripened into friendship, and a correspondence ensued in which Hamilton sometimes acted as critic of a poem by *Speranza*, at other times confided

to her the story of his life. The same reasons which have led me to refrain from presenting in print Hamilton's correspondence with the Nichol family dictate a similar course in this instance. But it would be wrong to leave unrecorded the fact that Hamilton was not only interested in the mind of this gifted countrywoman, but esteemed highly her whole nature, in which he recognised many features of native nobility.

It was at the end of 1855 that Hamilton was led by his study of the Almagest of Ptolemy to engage in computations connected with the *Problem of Hipparchus*, which set forth the remarkable accuracy of the observations and deductions of Hipparchus; a degree of accuracy declined from, rather than improved upon, by his admirer and commentator of 300 years later date, Claudius Ptolemy. A specimen of this exercise of Hamilton's powers in exhibition of the work of these Fathers of Astronomical Science may be found in *Hermathena* for 1883. By his study of this astronomical problem Hamilton was led on to investigate, in an extended treatise, a geodetical problem, having much affinity, which had been proposed and solved by the celebrated Snellius in 1617. This treatise remains unpublished. Among Hamilton's scientific correspondents during this year were, besides De Morgan, Lloyd, Boole, Cayley, Carmichael, Romney Robinson. I find four pages (61-64), but no more, of a letter to Mr. Townsend, F.T.C.D., on Co-planar (or Homosphæric) Involution.

An extract from a letter to Mrs. Wilde, with which I introduce the non-scientific correspondence of this year, has much biographical and ethical value, as showing that though Hamilton loved praise he also feared it, and that his was a mind which, with regard to politics, was no holder of stereotyped opinions, but capable of growth, and of comprehensive sympathy. After some remarks (prompted by a letter from his enthusiastic correspondent) on the dangers of the reciprocal admiration of friends—its liability to degenerate into reciprocal flattery—Hamilton observes:—

From Sir W. R. Hamilton *to* Mrs. Wilde.

'Observatory, *June* 30.

'. . . At all events you are no *chaplain* of mine, but only a fair *compatriot*, whose Irish feelings prompt you to take the most favourable view in your power of one who is essentially an Irishman by birth, and life, and labour, though educated by a clergyman who held the ascendancy principles (from which, by very slow degrees, I have been through life gradually emancipating myself), and who would have regarded repeal as rebellion. It was English history, not Irish which I was taught; and my heart still throbs with sympathy for that great British Empire to which, from childhood, I have been accustomed to consider myself as belonging as to my *country*—though Ireland, *as* Ireland, has always been the object of my *love*—and, I think you will admit, of my exertions. God forbid, then, that I should even smile, and I pray you also not to think it, at any grief, or sadness, or depression which any feeling of patriotism may have caused you. Education has much to do with the direction of such a feeling, but I know what it is, without hope or fear of any private gain or loss, to sympathise with a *nation*. I was almost literally sick with sorrow at hearing of the disasters in Cabool and the Khyber Pass several years ago.'

Passages from a letter written in the spring of 1885, to his old and kind friend, Lady Dunraven, will, by the mention of the sister of Aubrey De Vere, lead on the reader to the letters which passed in this year between him and Hamilton.

From Sir W. R. Hamilton *to the* Countess Dowager of Dunraven.

[from a copy.]

'Observatory, *May* 19, 1855.

'How often have I poured out to you in years now long past the secrets of my heart, and how kindly you suffered me to do so, and responded to, and comforted me!

> "Lady, who with a mother's tenderness,
> And fond indulgent patience, nursingly
> Cherished that hope in its frail infancy. . . ."*

'You see that I quote from memory [alluding to a verbal correction made in his writing of the quotation], though I don't forget your most kindly returning me some years ago the original, such as it was, of those that were at least *heartfelt* lines.

'A very unexpected and yet very natural circumstance, or train of circumstances, has brought back Ellen De Vere, and of course you, dear Lady Dunraven, with unusual freshness before my mind and heart, within the last few days, although you and she had never been forgotten.

'My dear sister Eliza, the poetess, whom I am ashamed to confess that I forget whether you ever saw, but with whom Lady Anna Maria exchanged some friendly notes, died in my arms in May, 1851, having every comfort, spiritual and temporal, which it was possible to procure for her, and with that last poetic satisfaction of the evening sun shining beautifully and gloriously, as well as comfortingly, in for the few minutes which were her last. We read of the Sun of Righteousness, and the early Christians accepted the title of *Sun*day from the heathens as associated with Him who, on that day, rose from the dead, and triumphed for Himself and for us.

'My sister Eliza left me the most entire control over her papers, which were many, with power to preserve or *destroy*. To some extent—indeed to a large one—I have used the *latter* power, by putting into the flames, after reading them, a great number of the sheets of a journal which, I am convinced, would have brought me in some hundreds of pounds if I could have borne to publish it, but which was written for *herself alone*, though in her dying hours she gave *me* permission to read it, and which described too freely, as I judged, though without any particle of malice, our many visitors of long ago to this Observatory.

'But among the numerous papers which thus came into my hands, it is only *very* recently, after the expiration of *four years* of mourning for my sister, that I have taken courage to open what she considered her *pet box* of *letters*, and had confided unreservedly

* Vol. i., p. 510.

to me. If I tell you that I found in that box a large number of letters of my own, you will make allowance for the affection which we felt towards each other, and which was more like that of twins than of a brother and sister whose ages differed by more than a year, but who "in early childhood, almost infancy, had wandered forth together fancy-fraught."* We loved each other to the last, and I had the satisfaction of being . . . *useful* to her. Indeed I have burned a great number of letters of hers to me, because I did not like to preserve records of the *gratitude* of one who deserved *all*, and *more* than all, which it was in my power to do.

'But among the papers of my sister I have found many letters which had not been written by myself; and among those I must confess that I have found, and even read, though I have not yet had courage to read through all of them, several letters from Ellen De Vere, and from Mrs. Robert O'Brien. At this great distance of time it did not seem to me an unpardonable sin, nor even any want of delicacy, to do so; and I was comforted on that score by finding in one of the most beautiful, but also most secret, letters of the set the remark: "I should be glad that *no person out of your family* should know of this letter." My sister, no doubt, feared to agitate me by showing me at the time that very charming letter; but surely I may accept, as an anticipatory *forgiveness* for reading it, the sentence which I have quoted.

'We are told from the pulpit, and cannot, perhaps, be told too often, of the faults and corruptions of our nature. Most fully, most heartily, do I grant all *that* from the experience of *myself*. But I must say that my opinion of the *good* part of human nature, and especially of the nature of young ladies, has been very decidedly *exalted* by the perusal of my sister's correspondence, not quite completed yet. How much it struck, how much it *affected* me, to compare the ardent yet discriminative enthusiasm with which Ellen De Vere and Dora Wordsworth (whose grave I kissed by moonlight at Grasmere last autumn) wrote to my sister separately of their first meeting!'

..

* Vol. i., p. 495.

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'KILLARNEY, *July* 25, 1855.

'. . . I cannot say how much I am obliged to you for them [letters of Sir W. R. Hamilton forwarded from the Seven Churches]. They are convincing proofs that you have not forgotten old times. To me old times seem rather to approach one nearer, as one advances in life, than to recede into the distance. To Time may be said, in its degree, what Wordsworth says to a mightier Power—

"Thou takest not away, O Death."

The present it is, not the past, that looks like a vision, except so far as duty and sorrow convert it into a reality.

'The extracts which you sent me [from old letters of E. De Vere about Curragh] have for me a deep and touching interest, partly of a different kind from that which they have for you. At first I hardly understood what they referred to, or by whom they were written. Gradually the mist cleared away, and scenes which belonged to my boyhood, but which were the most touching I have ever known, came back to me with the strange pathos of the past. The very features of the beautiful face, referred to in the letter to your sister, seemed to look forth again from the long darkness—not, however, in the radiance of youthful life, but in the yet more beautiful stillness of death—flower-crowned, and with a sweetness that seemed cast down from the region of Immortality.*

'I should wish to hear something more of your sister Eliza. She died, you tell me, in your arms, and four years ago. Had she been long ill? I trust that her death was one of peace and joy, for I believe that there is sometimes a joy as well as peace in death that life knows not of. This, however, is granted or not granted, as God sees good for each several person, and no inference is to be drawn from its absence. I shall be very grateful for a copy of your sister's poetry. There are poems of hers which can hardly be forgotten. Those which you tell me my sister singled out for special approbation were, I think, my favourites also, but the *Columbus* I liked the best of all. Some of the lines of it

* This passage refers to a sister of the writer of the letter, who died at the age of fourteen.

recurred frequently to my memory during that long and trying period of change (progress I should rather call it) in my religious convictions, which ended in a change of my religious position—a period in which one seemed to navigate a trackless deep, over which Faith alone could be one's guide (though many a "floating wreath impearled" gave good omen), till were heard the voices—

"Deep-murmuring 'land' in music proud." *

'She must have left many poems unpublished. I have directed my last volume to be sent to you at the address you name. I should have wished to dedicate the volume to you, as some of the poems in it were included in the volume originally so dedicated. It would not have been suitable to do so, however, as several of the poems at the end of the volume express opinions with which I could hardly expect you to be in sympathy. The present arrangement of the poems is, however, but temporary, and those that originally belonged to you will, I trust, find their way back to you ere long in a new edition. . . .

'To return, however, from Poetry to Life . . . . I am very glad that you made a pilgrimage to see Mrs. Wordsworth last year. She is the most venerable woman in England. I visited her also since her widowhood. She always spoke in the most cordial terms of you, as did the great Bard. He used to say you were the only man who had ever reminded him of Coleridge. You were sorry, I am sure, for poor Hartley Coleridge. His sister, Sara Coleridge, who died some years ago, was one of my most intimate friends. She inherited no small part of her father's genius. Do you ever read his works now? Wordsworth seems to me to have much more accomplished his mission. . . . It has been a real gratification to me to find that your thoughts have been reverting so much of late to that Past in which my own find so commonly their home. The modern world, ever rushing into the future, is, I suppose, against all reminiscences on account of their "inutility:" but surely this is but in accordance with its usual materialism. "A painless memory of pain" will probably be part of the inheritance of the Blessed hereafter; and we are surely the better for anticipating here what, we hope, awaits us in

* *Columbus*, by E. M. H., see vol. ii., p. 698.

a happier sphere. All your recollections of my sister give me the same sort of satisfaction which they would give to her also, as preserving old kindness, without the alloy of old suffering—though suffering is less to be called the alloy than the fire which clears the metal from the alloy. It seems to bring her back to me as she was when a girl:

> "There's something comes to us in life,
> But more is taken quite away."

'It is as a girl that I chiefly like to think of her—when Poetry and Thought seemed to give her wings. She has been happy as a wife and mother; though life has brought its cares to her as to all who root themselves in it; yet I do not doubt that she would have been not less happy had she continued to float over the surface of life, barely touching it, as of old. Be assured that the feelings she expressed in her letter to your sister in 1838 are those she feels now. . . .

'As for the Extracts—which I have been reading again since I began this letter—it is a deep satisfaction to me to have them. They come to me like a strange, and yet near and thrilling voice, from the region which we call the Past, but which is past only in semblance, for whatever has been lives on in its influences, and is destined to live again.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *August* 4, 1855.

'As to our religious views, you rightly judge that we differ more than we once did. Even if *you* had stood still, which was (as I suppose) impossible, at the point of Catholicism which you occupied when we used to have the most frequent opportunities of conversing freely on such subjects, and when there was, I think, very little difference of opinion, if any, between us, yet I have not stood still. Yours has been, as I most fully admit, and feel, with respect to *yourself*, a vital *progress*, a real psychological development; you are now more consistent with your own instincts, and so far subjectively *truer*. On the other hand, if you knew my mental history, you would be apt to say, or if too polite to *say* it, you would *think* that *I* have been *retrograding*, for at least ten years, since about the time of Mr. Montgomery's change of

profession. The question of progress or retrogression, as regards myself, I do not choose to discuss: but the (mentally) historical *fact*, expressed in words which we may both adopt, is that while *you* have been becoming, or considering yourself to become, more Catholic, and certainly more Roman, *I* have been growing gradually more Protestant, and (as you are welcome to call it) more Anglican, than I once had been.

'We are therefore, with whatever regret it may be admitted on both sides, by a *twofold* motion, less in harmony now with each other than we were in former years, on the subject of our religious feelings, impressions, and convictions—that is to say, on the most important subjects in the world—and I see no probability of this state of things being changed. I suppose that in saying all this I hardly give you any information—none certainly for which your letters alone would not show me that you were already abundantly prepared.

'If then it be painfully evident to both, that under such circumstances there CANNOT (whatever we may both *desire*) be *now*, in the nature of things, or of minds, the same degree of *intimacy* between us as of old; since we could no longer *talk* with the same degree of unreserve on *every* subject which happened to present itself, but *must*, from the simplest instincts of courtesy, be each on his guard not to say what might be offensive, or at least painful to the other: yet WE were *once* so intimate, and retain still, and, as I trust, shall always retain, so much of regard and esteem and appreciation for each other, made tender by so many associations of my early youth and your boyhood, which can never be forgotten by either of us, that (as times go) *two or three very respectable* FRIENDSHIPS might easily be carved out from the fragments of our former and ever-to-be-remembered *intimacy!* It would be no exaggeration to quote the words "Heu quanto minus est cum reliquis versari, quam tui meminisse!" . . .

'I have been very much gratified, indeed, by the kind message which I have received through you this morning, from one of the *two* ladies whom I have loved the best in the world—and of whom it is a deep satisfaction to me to know, that one is happily dead, and the other happily living—namely, the objects of my very early, and of my later, but still youthful poems.

'That I should have *seen* the one before her death, and have

heard to-day through you, at least by a message, from the other, is a matter of quiet joy, and indeed of thankfulness. It seems to say to me:—"Your past life is rounded off; you have lived for half a century, and the *last day* of it has been made *happy* by a *recognition* from one whom you had so very long and so very well remembered. Let *that* suffice for the *past*, and try whether you cannot, for the future, not indeed exert yourself more vigorously (that were unreasonable and hopeless to expect), but at least work more calmly, suffering all agitations about former times on earth to be absorbed in thought of the future eternity." Decidedly I feel it as a *close* to an *era*, although a very *consoling* close to me, that on a day which to my own imagination, or perhaps fancy (for I am accustomed to have associations with, and to receive impressions from, *days* that seem to me remarkable), presents itself so much as one of *transition*, as that of *my* reaching the age of fifty, at the same time that my second *son*, Archibald, attains *exactly* that of twenty years old, I should receive through you a kind message of thanks from your sister, from whom I had not heard in any way for so very long a time—in fact, since 1831. . . .'

From Aubrey De Vere *to* Sir W. R. Hamilton.

'*August* 9, 1855.

'. . . These are the things that show us the marvellous depth and strength of the human affections: when we call to mind how commonly they are left without response in this world, or even if responded to, are opposed by invincible obstacles, we cannot resist the conclusion that our whole moral being finds here below but a *rehearsal* of that great part assigned to it hereafter. On earth there seems no proportion between the strength of the affections and the objects on which, or circumstances under which, they are exercised. Were our life an animal life, man would indeed be the most ill-assorted and incomplete of all animals. But this very incompleteness of our destinies, considered as creatures of time, takes its place among the many proofs that we are made for Eternity;—and doubtless our human affections burn with a fire some portion of which is derived from that Love which is to last for ever, and to be bestowed on no human object, except as known and enjoyed in and through God.'

From the SAME *to the* SAME.

'DINGLE, *September* 1, 1855.

'. . . I do not like, however, to let another post go out without sending you at least my most cordial thanks for a confidence which is in itself the best possible proof of the sincerity and fidelity of your friendship. It also assures me (and this is a subject of great gratification to me) that, seldom as we have met of late years, you do not distrust the permanence on my part of a friendship which I have always regarded as one of the chief blessings of my life. When we met first, it was at a time when I had formed few friendships; and for this reason, as well as others, I naturally grew into the habit of associating you with all those studies and aspirations which belong to growing youth, but do not desert our maturer life. Later experience has never tended to withdraw my thoughts and affections from what belonged to that early time; and even to the most intimate of my later friends I have often been tempted to say, as Charles Lamb does in one of his poems, "Why did you not belong to the old, well-remembered times?" . . .

'I will take care to communicate to my sister, when I see her next, what you so kindly, and with such affectionate and generous remembrance, say respecting her; and in the meantime I may safely assure you that she has never forgotten you, or remembered you with other feelings than those of the warmest, most grateful, and most admiring friendship.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *September* 3, 1855,
'*Monday Night.*

'So *you* wrote those lines to Burns's *Highland Mary*, which I had so very deeply admired, without having the slightest hope that I should ever know so much as the *name* of the author! Surely they were not printed in either of your two former volumes. I think it was in a number of *Blackwood's Magazine* that I first saw them, and very much they struck me; but I had not the least suspicion that they were by *you*. Perhaps such lines as

"Honour to Scotland and to Burns"

ought to have made me suspect that they came from a foreign

source—foreign, I mean, to Scotland; but I must own that I always attributed the poem to some native Scotchman. I congratulate you most heartily upon the authorship of it. To anyone whatever it would, in my opinion, be a fit subject of congratulation. . . .

[Here followed critical remarks on several of the poems contained in a volume published by Aubrey De Vere in 1855.]

'I must once more congratulate you, not merely on the poetical skill, but also on what seems to me the perfect taste with which you have expressed your new views, and the complete absence of bitterness, so far as I have yet observed, even in manner. The change seems to have been *no* change to you (though I can well believe your statement that it cost you a struggle). The *old* poems which you have retained appear to be quite *at home* among the *new* ones. Yet I think that, if you look back upon that volume which you dedicated to me (and of course you *could not* have properly paid me the same compliment *again*), you will understand that *I* may have almost entirely sympathised with you *then*, and yet be separated importantly now; as, for instance, with regard to the Blessed Virgin Mary, whom certainly I am not conscious of honouring *less* than I formerly did, but whom you now honour *otherwise*.

'This slight comparing of notes as to our own *feelings* and your *poetry* is not designed to draw on any discussion of dogmatic theology. But I remain, my dear Aubrey, affectionately yours.'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'DINGLE, *September* 7, 1855.

'It is a very great pleasure to me to find that you like the poems of my new volume which you have had time to read as yet; and certainly, considering how short a time you have had the book in your hands, you seem to have made yourself marvellously well acquainted with its contents, as well as to have read it in a most friendly spirit. It is curious that you should have chanced to see the poem on *Highland Mary*. It was written during a tour I made in Scotland in the *winter* of 1845. A volume of Burns was the companion of my solitary way. Both

for him and for the Scotch people, with their singular mixture of imagination and warm feeling, with their robustness, courage, and perseverance, I felt a great, though by no means unqualified, admiration. I wished to write something in honour of the bard and of his country; and it struck me that, by making "Highland Mary" the centre of the poem, and, as it were, a connecting link running through it, something of *heart* might find its way into the poem, as well as a certain degree of unity, without which it would "drag its slow length along" only to weary the reader. It was a sort of farewell tribute to a land in which I had met kindness, and from which I had received many impulses of thought and feeling. A *Farewell to Naples*, written in a different tone, you will find in the same volume. It is probably too severe, but ought at least so far to confirm what I say in praise of Rome and the other sanctuaries of Italy, as to show that that praise comes from one who does not fancy perfection in every city which is washed by the Mediterranean, and professes the Roman Catholic faith. By-the-way, I am particularly glad to find that you like the *spirit* in which I have touched (at the end of my volume) on points respecting which we should differ. My firm conviction that the Church to which years of thought made me believe it my duty to make my *submission* (to *join* a Church is a phrase I could never use) represents the authentic and permanent form of Christianity—nay, that she alone gives to the bodies which have departed from her whatever of clear, firm, and orthodox teaching they retain—this belief ought surely to produce no bitterness of tone in the expression of it. To leave such convictions *unexpressed* would, on the other hand, imply little regard or respect for others, or else very little faith in *truth* as the great gift to man, through which God is pleased to dispense His other gifts. It is, however, a mere absurdity to fancy that, by expressing such convictions in a rude or bullying tone, we can convey them to others, or that we have a right to be surprised at others still thinking much as we once thought. We have neither to impose our sentence as a *judge*, nor to plead in the spirit of an *advocate*, but to *witness*, in simplicity and in manly humility, to whatever of truth God has been pleased to impart to us, and then let that witness fare as it may in a world preoccupied and not zealous for *truth*, though eager enough for *knowledge*.

'To come back, however, to your remarks on my poetry. . . . Whenever you have time to go through the volume, you may be sure that any remarks you may make on it will be most acceptable and useful to me, and that whether or not they should prove as favourable as those of your last letter. There are two sets of poems on which I should especially like to know your opinion. The first is a series of poems on Ireland, printed consecutively, and beginning with some sonnets called *Colonization*. They conclude with a poem called *The Last Irish Confiscation*, which, being cast in a dramatic form, as the address of one of the early and dispossessed race to one of the conquering race, reduced in their turn, can be judged on purely poetic grounds, and with or without the aid of political sympathy. The note at the end of the volume is necessary to explain it. The second class is the series of love poems called *Psyche*. One of them, beginning, "Such beauty was not born to die," would, I think, have a special interest for you. My poems have been said, even by indulgent readers, to be deficient in *passion*. If so, I should think that the defect exists less in those two series than elsewhere. I have sent on your remarks to my mother and sister, who will be much interested by them.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *September* 10, 1855.

'. . . I have been as happy in my own marriage as I expected, and more than I deserved to be. My *three loves* have been of *kinds* entirely different, and were felt all along to be so. I do not think that I ever confounded the three feelings, though it might be tedious, and in some degree impertinent, or presumptuous, for me to pretend to analyse them now. In general I might perhaps be permitted to say, to you, that they sometimes suggest themselves to my mind, as having been *characteristically* those of a *lover*, a *brother*, and a *husband:* selecting, you know, what has been eminent in each of them.

'I ought to be packing up and otherwise preparing for Glasgow; but must add one line to thank you for having pointed out *Psyche* to my notice. I had feared that the poem was a long *allegory*, and had resolved to take my time about reading it. The thoughts and words are beautiful.

'. . . But as for me, I am going rather for amusement and to see people than to do anything important at Glasgow. Still I would go for the mere purpose of being there on Monday next during that single hour when the arrangements respecting the place of meeting for next year will be decided on; in order that, as a Dublin man, I may put in once more a claim for a visit of the Association to Dublin in 1857.'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'MOUNT TRENCHARD, FOYNES, *September* 18, 1855.

[Referring to a letter of old date from a friend, expressing admiration of Mr. De Vere's character, which had been sent to him by Sir W. R. Hamilton.]

'. . . Tennyson's new volume, *Maud*, contains these lines describing the feelings of a man, when, at his desire, the lady of his love, with whom he has quarrelled, gives him back his presents:—

> "As looks a father on the things
> Of his dead son, I looked on these."

Such are the feelings with which we are apt to look on memorials of early praise and early expectations. There are doubtless, however, some who can regard the mementos of their youth with less mingled feelings: those persons especially of whom Wordsworth says that they "have wrought upon the plan that pleased their childish thought," and whose earliest aspirations have brought forth sixty-fold or a hundredfold. The *chief misfortune* of my life I regard as having been the absence of the habit of Confession, owing to my not having been brought up as a Catholic—a habit which I regard as necessary in an equal degree, though for different reasons, for the most opposite characters—the weak and the strong, men of genius and the dull. Without it, sterile soils are cheated of the increase possible to them, and rich soils abound as much in weeds as flowers: our very gifts become special sources of temptation and aberration; and we are left to learn, by the experience of life, lessons which ought to have been the *foundation* of a manly and Christian life. Here is a little bit of condensed biography on my part in reply, or part reply, to a portion of your letter; for I speak from observation and experience, not theologically, and with reference to

the *human* aspects of the question only, as a psychological problem, without discussing the question whether or not a Rite of *Forgiveness* stands to post-baptismal sin, as Baptism stands to Original sin. . . . Yet no suffering is sent in vain. All trials find their place in that great scheme of Probation which is the *Reality* of Life in its present stage . . . and all that we have thought, and felt, and done, lives on in us to the end of our lives, furnishing new materials for a probation which is ever changing its character in the successive epochs of our life.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *October* 19, 1855.

'I found that it at times agitated me to a degree which was imprudent for health, of body and of mind, to write as I was doing before I went to Glasgow, on subjects that are still so very vividly remembered. My visit was an useful diversion of my thoughts . . .'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'*October* 20, 1855.

'. . . I hope you will go on writing sonnets, so as to make them embody a large proportion of your views and feelings on personal, ethical, and philosophic subjects; but I have a still more important work* for you which you must execute some day, and for which you have such great qualifications . . . Your visit to Glasgow must have been a pleasant one—and particularly useful at the time it took place; for there is always an agitating interest in a review of the critical passages of one's past life, as well as a deep moral interest that allays agitation. . . .

'. . . There is something very touching in what you allude to respecting your pilgrimage to the house in which you had first been greeted by the "beautiful Vision." But surely all such Visions should be looked on as Anticipations and Types of the Glory and Beauty unrevealed, rather than as lights which have melted away into the sad shadow-land of the Past. There are three Worlds—those of *Sense*, of *Faith*, and of *Glory*. The first is in some measure eclipsed, or passed by, for a Christian.

* Not here indicated, but see *infra* A. De V.'s letter of May 11, 1856.

The last is not yet revealed: but all the shapes on Earth that are shapes of Beauty and Brightness (flowers, stars, poetry, and whatever belongs to the Ideal) seem to me to be sparkles or flashes shot down *through* the dim atmosphere of the region of Faith *from* the region of Glory. They are *gleams* to remind us of a higher world, just as there are intimations from the region of Faith that transpierce the darkness of the lower region of Sense, and give a vitality even to paganism. It seems to me of the essence of these things that they should be but *gleams*, for the World that unites Brightness and Permanence is yet to come.'

From the SAME *to the* SAME.

'MOUNT TRENCHARD, *December* 20, 1855.

'. . . I am sending you a copy of a poem which has, over and above its poetic merits, an interest for us, Irish readers, being a memorial of a most promising young Irishman, the late Lord Belfast. I want you to tell me also *how you like it*, especially the singular effect of the triple rhymes in the proem. The author* has published also a volume of poems, many of which are very beautiful. . . .

Talking of poetry puts me in mind of your fair friend "who Speranza hight." She certainly must be a woman of real poetic genius to have written anything so beautiful, and also so full of power and grace as the poem† you showed me. For the sake both of poetry and old Ireland you must do all you can to make her go on writing, and publish a volume soon. Do not forget to tell her that you showed her poem to a stranger (a stranger always counts for something) who has been addicted to poetry all his life, and reveres it more every year, and that he felt a very sincere admiration for it; although in the matter of your proposed metrical changes, he had the impertinence to agree with *you*.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'*November* 14, 1855.

'I do not feel in the least afraid, for myself, of *reading* anything. I have read atheistical books, infidel books, Socinian books, Protestant evangelical books, books by the Archbishop of Dublin,‡

* Denis Florence MacCarthy. † *Shadows from Life*. ‡ Archbishop Whately.

Protestant High-church books, Romanist books; and remain a moderate member of (what *we* used to call) the Church of England: more nearly perhaps approaching to the Archbishop's school than when we talked most about such matters formerly, though retaining a sincere sympathy and reverence for some whom we called Puseyites, and indeed for Dr. Pusey himself, though I am more *conscious* now than before of being farther removed from Rome than he is. But I quite distrust my own power of expressing to you, with so much of personal delicacy as you have used towards me, any of those beliefs of mine which may, or rather must, be different from yours. Indeed we should not meet on equal terms: for while I could listen to *anything* from you (though I see no *use* in listening to it), you not merely might be, but (I should almost say) ought to be, offended if I were to express at all fully my views of the Romish Church. Merely in my calling it *such*, you have already ground for a very pretty quarrel! though I think you will not avail yourself of it this time.'

I now give extracts from the correspondence with Dr. Nichol and his son, commencing with a letter to the son:—

From Sir W. R. Hamilton *to* John Nichol.

'On board the "Ariel," *September* 26, 1855.

'. . . It will gratify me much if you shall sometimes send me copies or draughts of your poems. I do not pretend to be a poet, or in any important sense an original thinker myself, but if you knew more of my (very unimportant) private history, you would be better able to understand that, without giving up any one of my own convictions, I may be able to sympathise with young men who earnestly think for themselves, even if the process lead them quite up or down to the point of Atheism, to which it does not seem to have led you—and from which I recoiled.

'Allow me to repeat, in writing, what I half said in a recent conversation, that, much as I love and honour your father, my regard for *you* has a distinct and independent root; and that though I may prove a very bad correspondent to you (in the case of Aubrey De Vere there was a whole host of early recollections to be called into play, if our remembrance of each other in later times had flagged), any letter with which you may at any time favour me will interest me for its own sake.

'I trust that you cannot be displeased by my adding that if Miss Nichol, your sister, should ever favour me with the sight of any of her manuscripts, the kindness will be appreciated, and in some slight degree repaid by a free but friendly criticism.

'With regards to Dr. and Mrs. Nichol,' &c.

A memorandum, bearing date the same month as the above letter, records in the following words Hamilton's modest estimate of his poetical faculty:—

'1855, *September*.—I am just enough of a poet myself to be enabled thereby to sympathise with other poets, to enjoy habitually their successes, and sometimes to be, without unkindness, sensible to their faults, or short-comings, better than without some such exertions of my own (in the department of poetical composition) might have been possible for me.'

From the SAME *to the* SAME.

'OBSERVATORY, DUBLIN, *September* 29, 1855.

'My son Archibald (the younger of my two boys) made out, this morning, the cipher on the other half of this sheet, without his having previously the slightest notion of the subject, or even the language; but I had previously taught him my key. I shall perhaps ask him, after a day or two, to write out the same passage for you in some other form, consistent with the same *key* (not alphabet), and, like the present, definitely interpretable. You would be astonished, I think, and even your father, with all his knowledge of the powers of numbers, would be so, if I were to mention in *how many* distinct ways the *same* extract from your poetry could be written on my plan, without the *slightest* ambiguity of interpretation! A recent and hasty calculation leads to the conclusion that the expression of the number, on the usual decimal system, would require 230 *figures*. And even my own initials could be written in more than 300 millions of different ways, so that my son (for instance), or anyone else who had the key—the plan—which can easily be carried in the memory, and is so carried by me, would decipher, in a minute (all helps being ready), any one of those 300 millions of distinct monograms, as easily as any other of them, to signify precisely "W. R. H.", and nothing else whatever; and this independently of any arbitrary punctuation,

or introduction of astronomical, or mathematical, or Greek, or German, or Oriental symbols, such as might at pleasure be mingled with the characters of the English alphabet.'

From J. P. NICHOL, LL.D., *to* SIR W. R. HAMILTON.

'OBSERVATORY [GLASGOW], *September* 28, 1855.

'. . . My letter to you is, as you will find, little more than a fond recalling of the most pleasant week you spent with us. I assure you it is a week that none of us will ever forget. You taught us far more than your cipher, viz. how exquisite and lovable is the *highest* genius.'. . .

From SIR W. R. HAMILTON *to* J. P. NICHOL, LL.D.

'OBSERVATORY, DUBLIN, *October* 1, 1855.

* * * * * * * * * *

'The cipher on the other side seems to me likely to be found a difficult one to make out, so long as the *key* is not given, or at least the *plan* explained: an almost unlimited *flexibility* as to the *writing* in this cipher, combined with an absolutely rigorous *definiteness* in the *reading* of it, seem to me to be the chief characteristics of the method; . . . . I claim to be able to *teach* the method to any sufficiently intelligent person in something like five minutes: at least if I should be allowed to use a very simple *machine* which I have invented for the purpose of writing and reading the cipher, but with which I can upon occasion dispense. The *principle* cannot be forgotten, and the *key* can be at any moment reconstructed, or the machine *re-made*, with ease. . . . It occurred to me about ten years ago, but the war has since made it surge up into my thoughts again—for it might, perhaps, be useful on some occasion of a secret despatch. . . .'

From SIR W. R. HAMILTON *to* JOHN NICHOL.

'OBSERVATORY, DUBLIN, *October* 10, 1855.

'. . . When you know me better, you will be aware that I am an abominably bad correspondent, except now and then by fits and starts. To Aubrey de Vere I had not written for several years; but, within two or three recent months, he and I have exchanged letters, which would make (in size) a respectable little volume, though they were (and are) in no way designed for

publication, and indeed were in many respects confidential; for we wrote much to each other about the days of long ago, being at least twenty-five years acquainted, and having, from some circumstances of my history, become intimate almost immediately . . . You and he are pretty nearly antipodes of each other, in many important respects, yet I can most sincerely sympathise with each of you—each being earnest and sincere, and also cultivated and poetical. That neither is specially scientific may (who knows?) be another attraction; though, that I can honestly enjoy the fame of a scientific brother—if this last word may without presumption be used by me—will be seen, if I shall ever transcribe my sonnet to the deceased Fourier (author of the mathematical Theory of Heat, &c.), or one of those to the living Herschel. Still, the two names they mentioned are of men who have been much more than mathematical calculators: and your father has such a vast variety of cultivation, that one may be pardoned for sometimes forgetting that he is by profession (and of course in reality) a man of science. Professor De Morgan, of London, is again a person excessively unlike to all of you; yet he and I have exchanged a great number of pleasant letters, partly no doubt on mathematics, but we are not afraid to write nonsense to each other; at least I send him nonsense at times, and he sends me back wit in return, rising occasionally to humour. I wonder whether I shall ever venture to show you (if I can lay my hand upon it) that letter from Mrs. Flamsteed to the late Francis Baily, which De Morgan professed to find among the papers of the latter! Flamsteed, you know, was the first Astronomer Royal at Greenwich, and a quarrel between him and Newton has even recently become matter of discussion. Baily died only a few years ago, but he had taken Flamsteed's part in the controversy. Mrs. Flamsteed writing (if I remember) from 3, Paradise Row (or perhaps it was, from 3, Astronomer's Row, Paradise), tells Baily that her good man is just gone out to take a social stroll with Sir Isaac, with whom he has become quite reconciled; but that, as to *that* scoundrel, Halley, who had come between them, it is lucky for *him* that he had not come up THERE, for her husband would have beaten him within an inch of his life! (after all, Halley, whose name has been given to a comet, was in many respects a fine fellow.) I never write in that style myself, but certainly I enjoyed De Morgan's report of that celestial-

infernal letter—though I did not show it to my wife, who would (or at least *might*—I was not sure) have said it was profane. Still she has been much amused with many of De Morgan's letters, and I think would welcome him here.*

'If there be any logic at all in this gossip about my correspondents, or some of them, it tends to show that I shall be pleased to receive at any time a letter from you, whom it is scarcely praise, and certainly not flattery, to say that I regard as a remarkable young man. Had I only met you in a steam-boat on one of those extemporised committees about poetry, I persuade myself that I should have long remembered you. As it is, with all the associations of your father's fame, and recollections of my reception at his very pleasant home, I feel certain that I shall never forget you. Yet I am aware that it is TOO LATE for me to hope ever to enjoy with you that *kind* of friendship, which *might*, nay (I am sure) *would*, have resulted if it had been possible and permitted for us to meet as two young men. On that subject, I think that I must extract for you a passage from one of my friend Aubrey's letters. Meanwhile, you will just write to me as seldom, or as often, as you shall feel inclined; and will remember me with patience and good humour, even if I shall leave some interesting letter of yours for months, or years, unanswered, though I think that I shan't be *quite* so bad as all *that;* but I *do* take fits on me, at times, of solitary study, during which I cannot bear to write a letter, and then I grow ashamed to write at all. . . .

'About my cipher (only *one* of my boys as yet knows it—a cipher and a mystery seem to be congenial) you really deserve credit for your guesses. You are, what in some childish game is called "hot"—close on the scent; but have not quite caught the hare as yet. You deserve that I should enclose the translation—I do not say the *key*. In finding out the latter, I enjoy the thought of your having some trouble still!'

From SIR W. R. HAMILTON *to* J. P. NICHOL, LL.D.

'OBSERVATORY, DUBLIN, *October* 17, 1855.

'I don't know what puts it into my head, but I am in a mood to remember, and write down for you, the latter part of a sonnet

* See Professor De Morgan's *Budget of Paradoxes*, p. 188.

which I composed by starlight about seven years ago, in the high gallery of the great telescope at Parsonstown.* The former part of that sonnet has happily escaped my recollection, but I know that it was execrable. Certainly I did not intend to flatter Lord Rosse, to whom I had the honour of being introduced by his father a great many years before; but there was something of compliment, or at least of politeness, in my mood, at first, which ought not to be, or rather cannot be, the inspiring spirit of any true and genuine poetry. Yet I own that I rather like, on recollection, the last six lines, expressive of an astronomical enthusiasm, including also a feeling for natural beauty, into which my tone gradually deepened, and in which I know that you can sympathise. I remember that I was standing alone at midnight, on a sort of little bridge somewhat like sixty feet above the ground, while Airy, with Lord Rosse, was seeing, at a lower altitude, some newly-discovered spiral nebula.'

> 'Pursuing still his old Homeric march,
> Northward beneath the Pole slow wheeled the Bear:
> Rose over head the great Galactic Arch;
> Eastward the Pleiads, with their tangled hair:
> Gleamed to the west, far seen, the lake below,
> And through the trees was heard the river's flow.'

From the SAME *to the* SAME.

[FROM A COPY.]

'OBSERVATORY, DUBLIN, *October* 31, 1855.
'*All Hallows' Eve.*

'. . . I really must write again to Agnes [Nichol] to tell her a little more about the budding "authorship" of my Helen. It takes me quite by surprise, though it may never ripen to anything important for the world. She poured upon me, a few days ago, quite a stream of questions about your daughter: what *sort* of authorship she was engaged upon, and soforth; but all *that* may better be told to your Agnes herself. I assure you that a thought of my daughter's about the Sea and its "Despair," which (with

* Vol. ii., p. 620.

glowing cheek pressed to my own) she confessed to me last night, appeared to me to be *perfectly* original! It was at least entirely new to *me;* and yet it seemed so deep, but true, that if I had ever met with it in reading, I could not have forgotten it.'

In a later letter to Dr. Nichol, referring to this idea of his daughter's, he writes—'I know that there is a Latin motto "Conantia frangere frango": but still the thought of the sea's *despair* seemed to me new.'

From J. P. NICHOL, LL.D., *to* SIR W. R. HAMILTON.

'OBSERVATORY, GLASGOW, *November* 6, 1855.

'Would that I could adequately tell you how much I value your confidences, and how gladly I welcome—even at this comparatively late date in life—the approaches of a new friendship—to say nothing of such a friendship as yours! I have outlived much, far more than I can estimate in value of any kind; and it is something to find, when one is growing almost weary, and inclined to think oftenest of the future, and a final rest, that new companions may still be found to take place on benches that have been vacated, and willing to pull with one at the oar. But this is only the selfish view of the case. I look with still profounder interest on the events of which you have so kindly told me, because I would fain learn what those things are that have torn, tried, and educated a mind like yours. Is it not possible that when summer returns I may induce you to cross over to us again, and that we two shall talk of such things and much else, during a ramble through Kannock Moor, or by the margin of vast and solitary Loch Awe? It is in such places, I always think, that it is best for the mind to look into its own depths, and to hear of the sorrows and triumphs of another. Do keep this in memory. I shall do my best to induce Lady Hamilton to spare you for awhile then.

'I never heard of your daughter's rare idea, the "Despair of the Sea," but how many images it brings up. Tell her from me that she has added a new note to that grand music—a music which generally gives me a deeper sense of infinitude than even

the midnight deeps. Depend on it that the mind which could suggest or *find* that conception will find many more rare ones. Agnes sits opposite me just now, writing a chapter of *Hermione*. We are alone together in the green room. It is very late, and the clocks seem to tick loudly. . . .

'Mrs. Nichol—who has been a guardian angel to Agnes—is not quite well to-night, and has retired. But you are sure that her affection and respect always extend to you.'

From SIR W. R. HAMILTON *to* JOHN NICHOL.

'OBSERVATORY, DUBLIN, *Nov.* 12, 1855.

'. . . Any criticism on the "Maud" will interest me much, but especially any notes on that poem by one whom I do so strongly associate with it as I do yourself. Will you believe that it is only within the last few days I have read the poem here! or indeed properly speaking anywhere; but within those days I have read it several times; and think that it was not indolence which prevented me from procuring and reading it sooner, but rather the wish not soon to disturb the associations with my having *heard* you read it (or most of it) at your father's Observatory. Perhaps I might have reposed on that recollection for some weeks longer, if it had not been for the shock received on Monday last (November 5th) by the perusal of a clever but uncandid article on Tennyson's poetry in general, which appeared in the first number (for November 3rd) of a new London publication, the *Saturday Review*, a specimen whereof was sent to me. I had recently been reading *through*, and more than once, the "In Memoriam": you conceive, from the connexion, that I do not speak here of your lines so entitled, though I have lately perused *them* also with much interest, but of Tennyson's, whose *Princess*, and several shorter poems, I had also not long since read again. Consequently, I was the more prepared to form my own opinion, as I went along, about the justice and candour of the reviewer's criticisms; and if you wish to lash yourself up to some degree of indignation, I recommend you to expend sixpence on procuring the article . . . . One artifice of the critic is to treat Tennyson as speaking *in his own person* throughout—for instance, in *Locksley Hall*, and *Maud:* as if Shakespeare were to be identified with Iago! . . .'

From the SAME *to the* SAME.

'OBSERVATORY, DUBLIN, *November* 24, 1855.

'. . . I am glad that you liked the sonnet to Fourier. Fourier was (as your father will tell you) a true *poet* in Mathematics, and in the applications of mathematical science to nature (especially to the theory of heat). So was (though *not* Laplace) Lagrange, to whose memory I consider myself as having inscribed those essays on a general method in dynamics, for which I received long ago a diploma, with an enormous double eagle engraved, and the names of Nicholas and of Hamilton stretching about equally across the sheet, from the Imperial Academy of Sciences at St. Petersburgh, which body has sent me a large packet during the war. There is something inspiring and consoling to humanity in the friendly and appreciating hand thus stretched through battle across Europe! I astounded a Viceregal party by telling them that I had just received despatches from St. Petersburgh, and high people spoke, half seriously, of arresting me; but I appealed to Lord Carlisle, who was so polite as to take charge of, and afterwards return, a specimen of my Russian bulletins. When we next met, the Lord Lieutenant asked me whether I had since been engaged in an animated correspondence with St. Petersburgh? "No, my Lord," I replied, "my last despatches have been from Rome." "And your next," said His Excellency, "will be, I suppose, from China." I bowed, and had the grace *not* to answer, that "I should not be excessively surprised!" America noticed me very early.

'Egotistical all this, is it not? To be sure it is; and do *you*, in revenge, make your reply as egotistical as you can find it in your heart to do. You will write to a *safe* and not an *inquisitive* person, who likes to know something of the inner history of any one whom he thinks of with regard, but is quite satisfied with the suppression of *names*. . . . Affectionately yours.'

From the SAME *to the* SAME.

'OBSERVATORY, *November* 26, 1855.

'I hoped to write more fully on the logical question, but have only been able to give it a corner of my thoughts. The spirit of a remark made at the end of a recent note is that we can't go on

proving for ever; we must, it seems, *assume* some principles; and indeed in *each single* syllogism the *premises* of *that* syllogism are *given*, not proved. It is not *by* syllogism, but by insight and sagacity, that the general *rules* of syllogism *itself* have been established; though I am not prepared to deny that any argument whatever *may* be *stated* in the syllogistic form. That "domino" can, I suppose, in the masquerade of thought, be slipped over any one of the intellectual characters which figure at the ball; whether usefully or not is a different question. Whenever we *reason*, we are conscious that we *generalise;* some principle or *as*-sumption (the major) is, *or at least* MAY be extricated, as we feel sure, from the perhaps individual instance which we have first in view: we regard that *case* (if it be one) as a mere *illustration* or exemplification, of a *principle:* and then comes in the *sub*-sumption (the minor), whereby we apply what had been formerly generalised, and so arrive at a conclusion. But it does not seem to me *natural*, even for a long-trained thinker, to throw arguments into the Aristotelian form; which yet I would advise you to study very diligently, as I once did myself. My first science honour in Dublin was won in logic.

'I have just now noticed in that wonderful *Almagest* of Ptolemy—the world's astronomical Bible for almost 2000 years—the word συλλογισμὸς, applied to *mathematical* calculation. Hobbes, as perhaps you are aware, regarded *all* reasoning as a calculation. But I must have forgotten a whole Library!'

CHAPTER XLI.

VISIT TO CHELTENHAM. ICOSIAN CALCULUS. CORRESPONDENCE WITH AUBREY DE VERE. LETTERS ON MATHEMATICAL SUBJECTS.

(1856.)

DURING the first half of 1856 Lady Hamilton was seriously indisposed with illness of a nervous character, similar to that by which she was affected in 1840–41, and letters of her husband prove not only his natural anxiety on the subject, but his affectionate devotion to her of personal care and detailed consideration. He encourages her pastor, Dr. Sadleir of Castleknock, to enter into religious conversation with her, urging with truth and wisdom, 'I have known even the *body* benefited by a courageous reference to things of a higher life': he mentions incidentally, 'I have been walking nearly the whole of this morning with Lady Hamilton in the garden': and he remains in doubt till the very last day, whether he shall allow himself the anticipated pleasure of attending the Meeting of the British Association and being a guest of his old friend John Graves at Cheltenham, telling his expectant host (August 8) that 'Lady Hamilton has been really *very ill* for a good while past. She has often parted with me before, but for the last six months or nearly so, I have been a sort of *nurse* to her, and it is a great effort to her to part with me, at present, even for a few days.' This illness of Lady Hamilton, and the confinement to which it doomed himself, had undoubtedly an injurious effect upon Hamilton's own health and spirits. There are indications of this in his journals and letters, prompting in the mind of the reader uneasiness and expectation of some break-down. Happily he was able at the last moment, on the 10th of August,

to obtain a release from home duties, and arrive at Cheltenham before the close of the Meeting. He was thus enabled to gratify his desire publicly to manifest his feeling towards Dr. Lloyd by seconding Whewell's proposal that at the succeeding Meeting of the Association in Dublin his old friend should have the honour of being its President.

Mr. Grove, the eminent author of *The Correlation of Physical Forces*, was his fellow-guest in the house of his friend; and familiar intercourse with these brothers in science, and more cursory meetings with other distinguished men, had the happiest effect upon his spirits, affording him the enjoyment and stimulus of intellectual companionship; but the physical effects of the disadvantageous circumstances I have referred to were not to be escaped from, and showed themselves in a severe fit of gout, by which he was rendered unable to walk, and obliged to remain for a fortnight longer than he had intended a recipient of his friend's hospitality. By both host and guest this was considered to be the reverse of a penalty. Letters given below show how he feasted upon the contents of the rich scientific library of Mr. Graves, and, as in earlier times, received from his friend intelligent sympathy in his most recent researches, and hints for prosecuting them in new directions.

Very soon after his return home he forwarded to Mr. Graves two valuable scientific Papers: the first a criticism on Scheffler's book, *Der Situations Kalkul* (a work on the Geometry of Space), in which the author arrives, after long discussion, at a conclusion which in 1832 had been rejected by Hamilton as insufficient foundation for the required calculus; the second, an extension by means of quaternions of some propositions laid down by Gauss in his *Disquisitiones Arithmeticae*. Their general nature is set forth in letters to Mr. Graves, which are, however, too mathematical for insertion in this volume: the papers themselves will probably appear in an early number of *Hermathena*.

In the following month of October—the happy birth-month of both Conical Refraction and Quaternions—Hamilton imparts to

the same friend his discovery of a new Calculus, to which he gave the name of Eicosian (afterwards changed to Icosian), because specially exemplified in the case of the Icosahedron, though applicable to other polyhedra. This offspring of his genius was not indeed of equal note with those which have been named, but it is not unworthy of its parentage. Professor Tait says of it—'Hamilton has published but a page or two with reference to them [the systems embraced by this discovery], yet that little is enough to show the probability of their becoming, at some future time, of great importance in the study of crystals and polyhedra in general.' It was Mr. Graves who, during Hamilton's visit to Cheltenham, had re-excited in him an interest, long dormant, in the mathematical properties of these solids, and to Mr. Graves accordingly Hamilton first made known his discovery, in letters dated October 7 and October 17, of which the latter extended to fifty pages. A summary view of it was communicated by him, under date October 29, to the *Philosophical Magazine* for December, 1856, with the title *Memorandum respecting a New System of Roots of Unity*, and a fuller statement of the discovery was made by him to the Royal Irish Academy on the 10th of November, and is reported in the *Proceedings*. From these communications I extract the following passages, which will give the reader a general idea of the Calculus:—

'I have lately been led to the conception of a new system, or rather *family of systems*, of *non-commutative roots of unity*, which are entirely distinct from the i, j, k, of the quaternions, though having some general analogy thereto; and which admit, even more easily than the quaternion symbols do, of *geometrical interpretation*. In the system which seems at present to be the most interesting one [that dealing with the icosahedron and the dodecahedron] among those included in this new family, I assume three symbols, ι, κ, λ, such that

$$\iota^2 = 1,\ \kappa^3 = 1,\ \lambda^5 = 1,\ \lambda = \iota\kappa; \qquad (A)$$

where $\iota\kappa$ must be distinguished from $\kappa\iota$, since otherwise we should have $\lambda^6 = 1$, $\lambda = 1$. As a very simple specimen of the symbolical

conclusions deduced from these fundamental assumptions, I may mention that if we make

$$\mu = \iota\kappa^2 = \lambda\iota\lambda,$$

we shall have also

$$\mu^5 = 1, \quad \lambda = \mu\iota\mu;$$

so that μ is a new fifth root of unity, connected with the former fifth root λ by relations of perfect reciprocity. A long train of such symbolical deductions is found to follow; and every one of the results may be *interpreted* as having reference to the passage from *face to face* (or from corner to corner) of the *icosahedron* (or of the dodecahedron); on which account I am at present disposed to give the name of the "Icosian Calculus" to this new system of symbols, and of rules for their operation.'—*Philosophical Magazine.*

'This Calculus *agrees* with that of the Quaternions in three important respects—namely, 1st, that its chief symbols, ι, κ, λ, are *roots of unity*, as i, j, k, are certain fourth roots thereof; 2nd, that these new roots obey the *associative* law of multiplication; and, 3rd, that they are not subject to the *commutative* law, or that their places as factors must not in general be altered in a product. And it *differs* from the Quaternion Calculus—1st, by involving roots with different exponents; and, 2ndly, by *not requiring* (so far as yet appears) the distributive property of multiplication. In fact + and −, in these new calculations, enter *only* as connecting *exponents*, and not as connecting *terms:* indeed, no terms, or, in other words, no polynomes, nor even binomes, have hitherto presented themselves in these late researches of the author. As regards the *exponents* of the new roots, it may be mentioned that in the *principal system*—for the new Calculus involves a *family of systems*—there are adopted the equations,

$$1 = \iota^2 = \kappa^3 = \lambda^5, \quad \lambda = \iota\kappa; \qquad \text{(A)}$$

so that we deal in it with a new square root, cube root, and *fifth root*, of positive unity; the latter root being the product of the two former, when taken in an *order* assigned, but *not* in the opposite order. From these simple assumptions (A), a long train of consistent calculations opens itself out, for every result of which there is found a corresponding geometrical interpretation, in the theory of two of the celebrated solids of antiquity, alluded to with interest by Plato in the *Timæus*—namely, the Icosahedron and

the Dodecahedron; whereof the *angles* may *now* be unequal. By making $\lambda^4 = 1$ the author obtains other symbolical results, which are interpreted by the Octahedron and the Hexahedron. The Pyramid is, in *this* theory, almost too simple to be interesting, but it is dealt with by the assumption $\lambda^3 = 1$, the other equations (A) being untouched. . . .'—*Proceedings* of R. I. A.

The investigation by this calculus of the properties of the Icosahedron led Hamilton to devise a mathematical game, which he called the Icosian Game. A diagram was formed by the projection of this exemplar solid upon a flat surface: and the passing continuously by the connecting lines from one corner in this diagram to another became in the hands of the designer a suggester of possible sequences, cyclical and non-cyclical, which, amounting in number to 62,400 (33,600 being cyclical), were included in 520 formulæ, all by him set forth in tabular form. The game founded upon the basis of this idea was to be played upon a board, on which the figure was cut in grooved lines with holes at the corners. In these holes, marked by the twenty consonants of the alphabet, were to be inserted consecutively, according to named conditions, movable pieces numbered from 1 to 20. The practical objection to this game was that, while some of the problems which might be set to the player required for their solution considerable thought and ingenuity, the majority were too easy to be interesting.* He corresponded at great length on the subject with Mr. Graves, by whom he was put in the way of selling the copyright of the game to Mr. Jaques of Piccadilly, who paid for it the sum of £25, the only pecuniary reward ever accruing to Hamilton directly from any discovery or publication of his. It is to be feared that Mr. Jaques did not find the bargain to be a profitable one.

The calculus itself, and the problems of the Icosian Game,

* In a note to Dr. Luby, F.T.C.D., dated July 6, 1860, Hamilton writes—'I cannot resist the temptation of telling you that you and Dr. Hart did *not* solve my little problem of the Icosian Board yesterday,' and adds, that having had the intention of presenting a copy of his Board to the Common Room of the College, he was discouraged by its reputation of being too easy.

afforded scope to their author for an amount of labour well-nigh exhaustive in its extent, which fills many thick manuscript-books, and is calculated to excite a feeling of wonder in all who are capable of estimating it. The letters to Mr. Graves forming the original communication ought, perhaps, to see the light, particularly as the writer was prevented by the preparation of his *Elements of Quaternions* from fulfilling his expressed intention of sending to the *Philosophical Magazine* further communications on the subject.

Professor Tait concludes his notice of the Icosian Calculus and the Icosian Game by the comment—'the analysis employed, though very simple, is more startingly novel than even that of the quaternions. The i, j, k, of quaternions can, as we have seen, be represented by three definite unit-lines at right angles to each other. How can we represent geometrically the λ or the μ of this new calculus, either of which produces precisely the same effect, whatever edge of whatever face of the dodecahedron it be applied to?'*

By what has been now sketched of the incidents of the year, the reader will have been prepared for the correspondence referring to them; but I give their proper precedence to letters which passed in January and February between Hamilton and Mr. De Vere. The interest of these letters is literary and personal, and the frankness, the open-heartedness of both writers, will, I think, be felt by every reader to afford the best proofs of the mutual trust, the reality and depth, of their very beautiful friendship. The reader will be best pleased by taking note of these proofs himself: I abstain, therefore, from forestalling his pleasure by anticipatory indication or comment.

From Sir W. R. Hamilton *to* Aubrey De Vere.

'Observatory, *January* 1, 1856.

'. . . I have read the Essays [*Essays, mostly Theological*, by A. De V.] quite through more than once, and many parts of them

* *North British Review*, September, 1886.

with admiration; but the chief feeling which they excite in me, or the prevailing impression that they leave, is the old "Talis cum sis, utinam noster esses," which, perhaps, you at times reciprocate. Your present views seem to suit *you* at least, and you write a capital *style*. But this must sound to you irrelevant, perhaps irreverent.

'Many thanks for returning my manuscripts. Your last conversation almost tempted me into writing again on the *potentialities* of the past—an idle, if not a dangerous theme—yet one to which the mind will recur.'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'CURRAGH CHASE, *January* 4, 1856.

'. . . I send you back Speranza's most amiable letter. It is indeed pleasant to meet that rare thing, poetic genius, in union with a rarer one—the magnanimity (in which genius is so often deficient, and without which it almost ceases to be respectable), which can take censure with gratitude, praise with simplicity, and both with equal grace. I am much pleased at the terms in which she speaks of my father's poetry, because I think them deserved. Wordsworth, a critic hard to please, thought his sonnets among the best in the language, both as to matter and composition. Notwithstanding, he remains almost as unknown in this country, to which he was deeply and loyally attached (in spite of his love of England, and dislike of popular agitation and such patriotism as is only Pat-*Riot*ism), as if Ireland boasted poets as numerous as blackberries.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *January* 15, 1856.

'I am in your debt at least two letters, and have at this moment to thank you for sending to me the newspaper containing a sketch of your very interesting lecture on General Literature, which, on the whole, appears to have been ably reported, though slight typographical faults occur: for instance . . . These things, you know, will force themselves upon a practised eye. Your own published volumes (and *I* may be excused for reckoning that you

have laid more than a grain or two upon the hills of gold) are singularly free from such faults; and I too have, or think myself to have, contributed a smaller proportion of typographical errors to my work on quaternions than is confessed by any equally conscientious mathematical author: for example, by Professor De Morgan.

'To turn for a moment to something higher. You know that I am not a flatterer; but I must say that I regard your character as nobler than my own. I might easily expand upon this topic, which the slight praise given to you publicly of late has so little suggested, that it seems to me painfully inadequate. But with the same sincerity I must say, that the *modern* Roman system appears to me so corrupt that the inevitable *tendency* of any other system, such as Puseyism, to it is, with me, a "reductio ad absurdum."

'I do not admit that I am a "touchy" person; the pain, and often the meanness, of such a character I count myself to have, through life, escaped. Yet I am glad that you have *not* tried my temper, by offering to send me stamps for the prepayment of your volume of collected Essays through the post.'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'CURRAGH CHASE, *January* 24, 1856.

'The volume of my Essays reached me quite safely; and I can assure you that, notwithstanding my joke about the stamps, nothing was ever further from my intention than to send them to *you*. There are, however, persons to whom I would have sent them—regular "nineteenth century" persons, as I should call them. When I was in Greece, I chanced to be acquainted with two people of good position at Athens—one an American clergyman, the other a Scotch lawyer. They were brothers-in-law. The Scotchman, of course, was "canny" in his way, but much more for others than himself; and while devoting all his energies to the cause of Greece (he had fought for that country, as well as laboured for the education of its youth at a later period), remained always as poor, not only as a field-mouse, but as a field-mouse on a Scotch heath. The American was also an excellent man, well-to-do in the world, with a fine house, comely wife, high position

in a little world of fashion, large reputation, &c. &c. Well, fancy this man confiding to me one day that his brother-in-law was not quite the right thing! "Would you believe," said he, "it is now two months since I had to pay a penny for him one day, as we were out riding together, *and he has never repaid it yet!*"

'When I was in Scotland, I perceived at once, even in the Lowlands, that Scotch worldliness is not like American or English. A Scotchman has warm and deep feelings; and if he is selfish, it is, in part, because out of the abundance of his love-power he has a great deal to spend on himself *as well as* on his friends. There are those who care nothing for themselves, and care as little for their neighbours. Moreover, the Scotchman, however thrifty he may be, has large sympathies, and a strong imagination too. He has reverence for antiquity; loves old kings and chiefs even more than modern lords and ladies; preserves old legends; imagines a something *venerable* even in that shrill-voiced demagogue, John Knox; sings the songs of Burns on moor and heather; really loves his country, and remembers its ancient fame when he looks on Edinburgh Castle, if not, alas! when he looks on Melrose Abbey; is capable of being profoundly attached to a *cause*, as well as to a person, and in defence of either bears the beating of the worldly storm, as the northern side of his native pines bears the sleet-blast and the clinging ice. In short, in the midst of his coarser nature there is a finer one also, the grain of which is delicate as that of those woods of which musical instruments are made. In many of these gentler qualities he is excelled by the Irishman: but then he has—what we lack—a robustness like that of the canvas used for storm-sails.

'I should like to know why I am writing you a disquisition on Scotchmen. I suppose it is because I *intended* to say a few words on theology, in answer to your remark on a tendency to *modern* Romanism being a "reductio ad absurdum," just as once before I wrote a theological letter to you when I meant to write one about literature and poetry.

'*Friday, January* 25. P.S.—My letter was late for the post. . . . My vindication of Scotland was, I suppose, a vindication of her from an imaginary charge—that of her being implicated in the ordinary "nineteenth century" character, with which she is

commonly, but most unjustly associated, even more than England. I might have remembered, however, that I had done what I could in this way in writing my *Highland Mary*, to which you had done so much more than justice. In a large part of my poetry, indeed, my purpose has ever been to vindicate some just cause, or to write the epitaph of one unjustly trampled down, and scrape the moss from neglected tombstones.

'Now one word as to the "reductio ad absurdum" implied in a tendency to modern Romanism, *i.e.* Romanism seen through the mist of *modern* associations, party impressions, social antagonisms, &c. I will only say that nothing of *this* kind can be taken by a philosophic mind like yours for more than, at most, an *impression* as distinguished from *opinion*, and must even be contrasted with *conviction*.'

Mr. De Vere then fills fourteen closely-written pages with a very ably-argued contention, that the charges brought against the Roman Church were necessarily incurred by a spiritual body, composed of sinful human beings, acting through many successive centuries upon a sinful world, and that they were of the same kind as the member of the Church of England felt to be unjust when brought against his Church by deserters. He concludes his letter by turning from the general argument to the following personal application of it:—

'I do not want to urge a new set of *à priori* impressions on you, in place of those so common among us, and to which I was once myself servile indeed, though I suspected it not. That would not be fair. I only want to point out the absolute necessity of shaking off all such vague *impressions*, and all that is not really and truly theological and philosophical in our method of thought, if we would reach truth on the most important of all points, and discriminate between the authentic and the spurious forms of God's Revelation. I have observed that those sorts of *impressions* and social traditions have often even more influence on the thoughtful than the thoughtless, if they be not on their guard against them, or if they fight the battle of theology with the left hand, while more earnestly interested in political or philosophic avocations. Once more, adieu. Yours affectionately.'

It could not reasonably be expected that Hamilton, with such work on his hands as the writing of his *Elements of Quaternions*, and with his thoughts absorbed in it, could find time or freedom of mind sufficient to enable him to grapple with so accomplished and earnest a controversialist as his friend. In his reply, accordingly, he is content with intimating that his previous convictions remained unshaken, while candidly admitting the fact that he was conscious of having his share of bias and prepossession.

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *January*, 31, 1856.

'. . . I like what you say about Scotland, and am not offended by your remarks on Theology—though not at all afraid of your succeeding in con- or per-verting me. In my *Lectures on Quaternions* there occur all sorts of "Versions"; such as *Re*versions, *Pro*versions, and *Trans*versions, with reference to *rotations* in geometry; but one of them is a *Non*-version! Even mathematics, you see, may supply a joke sometimes—such as Pope somewhere says, does he not? that "gentle dulness loves."

'Of course I admit that early *impressions* must in my case, as in most others, leave a very lasting trace—and that I do not pretend to be *unbiassed*—as indeed who can be? . . .'

In rejoinder to the above, Aubrey De Vere discharges a few Parthian shots, to be met with towards the conclusion of the following letter, the preceding part of which I insert on account of its interesting reference to his own poetry, and his, as it appears to me, admirable characterisation of the poetry of Sir Henry Taylor.

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'CURRAGH CHASE, *February* 1, 1856.

'. . . There is no quality I should be better pleased to see attributed to my poetry than *Passion*—partly perhaps because some not unfriendly critics have thought that its chief defect was

a *lack* of *Passion*. Henry Taylor suggests this in the form of a *query*, in a letter which assuredly is complimentary enough, and which, if I can find it, I will send to you (but you must let me have it again), though you are not to suppose I believe in all the kind things he says also of my verse. I was so ungracious as to answer his question by saying that I doubted very much whether it was in Passion that my poetry was deficient: but an author is the worst judge of his own works. . . .

'As for the Versions and Non-versions, I will give you, as a pious task, the *Con*version of our friend Anster on the subject of the Poet I have named to you, Henry Taylor. He is flinty-hearted as to him, which always amazes me: for *Philip van Artevelde* seems to me the greatest dramatic work since Shakespeare; and his other poems are admirable also. The degree of compact *strength* they exhibit is almost a miracle in these soft effeminate days, when few could claim the praise old Ben Jonson gave to a friend of his of whom he said, "my son Cartwright *writes all like a man*." To this H. Taylor adds a peculiar delicacy and fineness, of double value when united with strength, and a deep vein of genuine Thought, and a certain "noble and stately grace" which I should call the *differentia* of his muse. . . . Anster needs conversion also very much respecting Tennyson, respecting whom he is but indifferently affected.

'I must not serve you up any more Theology at present. I will only beg of you to remember one thing, viz., that however *natural* it is for us to be *biassed* by early training, or the "Public Opinion" about us, respecting Religion, and also to act on *Impressions* as though they were *Convictions* deliberately arrived at, yet, if there be any one thing *absolutely certain*, it is this, viz., that *Duty*, *Honour*, and *Safety* all alike imperatively require that we should, as far as we are able, rise superior to this natural tendency, which, even were it excusable in any, would not be so in persons of deeply thoughtful habits. All this belongs to the *Will*, for which we are plainly responsible; and if we believe in Christianity at all, surely it must be our first duty to discriminate with perfect *fairness* between the authentic and spurious versions of it. After all, surely consistent and logical thinking is but *honest* thinking on such a subject; and to many may not this be their *chief, if not their only, probation?* Affectionately yours.'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'OBSERVATORY, *February* 12, 1856.

'I have thought myself permitted to make a copy, in one of my manuscript-books, of Henry Taylor's remarks upon your poetry, all of which I think that I can adopt. Indeed most of them, or at all events many, had occurred to myself before. Perhaps I may write to you again upon the subject. At this moment I am detaining the morning postman, who is not bound to *wait* for letters, but whom I have bribed by promising him a cup of tea in my hall. He has brought me a note from Dr. Robinson, enclosing four stamps in repayment of a recent advance of mine, so if you *had* sent me stamps for posting your book (which I am very glad that you did not), you would not have been the only Irishman who has done something of this kind. I am rather vexed by receiving these stamps, but suppose that the civil thing is to retain them. I had put the same number of heads on a printer's packet which came to me by mistake a day or two ago. You will return me Robinson's note. He is a very pleasant person, to love or to quarrel with.* I have known him since I was a child, and we have had our little quarrels now and then. You and I never got up one! Affectionately yours.'

Although Hamilton declined to enter into written controversy with Aubrey De Vere upon the subject of Romanism, it would appear, from an entry in one of his manuscript-books, that his friend's arguments had led him to formulate, under the name of theses, some of his convictions in reference to the Church of Rome; and I think it right here to print them, for the purpose of showing that his resolute adherence to Protestantism was founded not

* What Dr. Robinson at this time thought of Hamilton is shown by a note written by him about a month later in reference to something said by Hamilton at a Meeting of Council of the Royal Irish Academy.

'OBSERVATORY, ARMAGH, *March* 8, 1856.—There was not the least occasion for you to suppose I could be hurt by what you said. I know your kind and good heart too well, and rely too firmly on our old friendship, ever to admit the possibility of your harbouring an unkind thought towards me; and I took [it] as one of the *points* to which an extempore speaker is occasionally tempted.'

solely upon traditional views and prejudices, but upon historical study and reasoned conclusions. At p. 349 of Manuscript-Book D. 1855, I find the following :—

'On ⊙ evening, July 6, '56, I wrote out for myself, and showed to Helen, with some dim purpose of hereafter letting A. De Vere see them, a few remarks in the form of *Theses on Romanism*. They were nearly these:—

'I. The Roman Pontiff is not the Vicar of Christ; nor is there any human Head of Christendom.

'II. The Roman Communion is not the Holy Catholic Church; nor is it even an incorrupt branch thereof.

'III. The Roman System is unscriptural; and is expressly denounced by anticipation in the Scriptures.

'IV. The Roman Worship is idolatrous in tendency; and its followers are in peril of the sin of actual idolatry.'

The absorbing nature of his mathematical researches is graphically expressed in the following extract from a letter to Aubrey De Vere. It renders account for his not giving an unconditional promise to look over a scientific Paper written by a gentleman introduced to him by Mr. De Vere; but it will show also how entirely his pursuits and his duties disabled him for discussing adequately by letter so great a controversy as that between Protestantism and Romanism.

'*July* 18, 1856 . . . In fact it is impossible for me to give any *promise* of that kind, because when a whirlwind of original investigation seizes me, it carries me away, and for the time my place knows me no more—I am out of space, and more than "*prope* mancus ad officia." And then I have all sorts of duties, already imperfectly discharged, and which I must not unnecessarily complicate.'

The following passage in a letter of Aubrey De Vere's, dated May 11, 1856, brings into view a project, entertained long before by Hamilton, of extending Bishop Butler's analogical argument to a defence of the Church, and also a striking similitude conceived

by him, at a still earlier time, in which a phenomenon of nature is made to illustrate a Christian doctrine:—

'. . . I well remember the discovery of Conical Refraction, though, alas, little able to appreciate these high things in science, much as I venerate them. That discovery, however, particularly impressed me at the time, as such predictions in science, like prophecies in religion, have a special value of their own. I wish some competent person would write an essay on *all* the analogies or "similitudes in dissimilitude" between the Sciences you are addicted to especially and Theology. I remember Manning (a deeply philosophical Mind) observing to me soon after he had become a Roman Catholic—(1) that it was not, in his judgment, *possible* for *Christianity* to have expanded itself into any large and consistent scheme of *thought*, such as is implied in the idea of revealed *truth*, except in the form of Catholic Theology; and (2) that next to this wonderful fabric of supernatural science (as set forth, for example, in St. Thomas Aquinas), the thing that he most venerated was the marvellous pile of Inductive and Mathematical Science, which was a development not less faithful of their own Laws of Thought. He spoke as if the two things, considered in relation to intellect alone, and apart from the soul, might be regarded as the twin summits of that mount

> "which doth divide
> Into two ample horns his forehead wide."

Now, there is a subject for you to write an essay on, by way of following up a scheme of thought which had presented itself to you before the days of Conical Refraction, when you spoke of writing an Essay pointing out the complete relevancy of Bishop Butler's mode of reasoning to the question of the Church, as well as to that of the Gospel. . . .

'Here is another plan for you—if you like poetry better than prose at least. I often wish you would write a series of *philosophical* sonnets. The condensed form of the sonnet makes it particularly valuable, as a sort of chalice for some *one* great and fruitful thought which deserves to be thus brought out. Again, at the various periods of human life there is a peculiar interest,

personal, as well as philosophical, in looking back with a view of ascertaining what are the most precious spoils which a certain quantum of meditation and experience has left with us, and which we should like to bequeath. I will mention a thought worthy of a sonnet, which will show you that I remember past times as well as you. Many years ago, when we were walking on the pier at Kingstown, you remarked on the contrast between the vast pacific expanse of a bright and level sea with the soft thunder of the breakers, which fell with perfect regularity, but drawing down a weight of ruin which must have destroyed any boat which *put itself in the way* of such destruction. You remarked on this contrast as illustrating at once the infinite mercy and infinite justice of God, and showing how Wrath and Vengeance are consistent with a Being all Love, and above all Passion. Now, in these days, when people, who mistake themselves for deep thinkers, as well as philanthropists, fancy that they can recommend Christianity by expurgating it of its doctrine of "Eternal Punishment," and render it more acceptable by divesting it of what, when superficially regarded, becomes part of the "scandal of the cross," you would do real good by treating the subject in a higher, though less popular spirit, and the sonnet form would answer well. Perhaps several sonnets might be necessary. . . .

'I have no doubt you are quite right in thinking that no correspondence is desirable. The more tranquil we can keep our spirit, the better will that still lake reflect the image of heaven, and the less will be stirred up of whatever can dim the waters. Besides, the past is a delicate thing, perfect in itself, but easily spoilt.'

The letters which follow relate principally to Quaternions and to Polyhedra, but to them I prefix a letter to Dr. Robinson, of an earlier date.

From Sir W. R. Hamilton *to the* Rev. T. R. Robinson, D.D.

'Observatory, *March* 24, 1856.

'My dear Dr. Robinson—Within the last few days I have received from Paris a quarto of about 200 pages, almost entirely

devoted to the development and application of my results in physical astronomy—the first part relating to my abstract results in dynamics, and the second being headed, *Thèse d'astronomie. Application de la Méthode de M. Hamilton au Calcul des Perturbations de Jupiter*—by Saturn, Uranus, Neptune and Mars, the Earth, &c. —with long inequalities of all sorts extended to the years of our Lord, 2300, and 2800—all by Prof. or Monsieur Houel, of Alençon—but submitted to Cauchy, Duhamel, and Delaunay, and (as it seems) approved by them. How comfortable to see my abstract results translated into hundredths of seconds sexagesimal! and how odd a feeling it gives to read, in the astronomical department, every now and then, of "l'ellipse de M. Hamilton"! or better still, here and there, without the "M," "l'ellipse de Hamilton"!—for it is the truth, though perhaps scarcely two or three persons in these countries have noticed it, that I assigned, twenty years ago, elliptic orbits for *all* the planets, essentially distinct in theory, though very little differing in practice, from those so beautifully imagined by Lagrange, and having certain centrobasic and symmetric advantages.* . . . I remain, in haste, but wishing to *consult* you on some things astronomical, most faithfully yours.'

From SIR W. R. HAMILTON *to* REV. HUMPHREY LLOYD, D.D.

'OBSERVATORY, DUNSINK, *September* 9, 1856.

'. . . I returned to this place only a week ago, having lingered for a quiet fortnight in Cheltenham, after the business and amusement of the Association week; which with *me* extended from Monday the 11th, to Saturday the 16th of August, for I was asked to several dinners, and visited, after the formal meetings had closed, the College, and other show-places, such as Lord Northwick's† splendid picture gallery, during that time. Conceive me shut up and revelling for a fortnight in John Graves's Paradise of Books! of which he has really an astonishingly

* See vol. ii., pp. 112–117.

† I happened to be introduced to his Lordship, who is now old, and he told me that he well remembered the English Sir William Hamilton at Naples, and showed me a miniature of Lord Nelson and a gem of *his* Lady Hamilton.

extensive collection, especially in the curious and mathematical kinds.* Such new works from the Continent as he has picked up! and such rare old ones too! Besides works of Archimedes and Apollonius, which I had read before, he showed me the original edition of Copernicus's book *De Revolutionibus*, &c., containing an *apologetic preface* by the editor, which Graves had not observed, and which, while modestly putting forward the motion of the earth as an *hypothesis*, endeavours to deprecate by anticipation the displeasure, not of the priests, but of the *philosophers!*—for no fear seems to have been as yet entertained of awakening the wrath of the Church; and indeed I believe that the work was *dedicated* to the Pope of the time, but am not quite sure of this.

'To descend to more recent times—though on my way to them I lingered for a good while on a charming folio of the works of Wallis, written in part in English first, but afterwards translated into Latin, for the greater ease of the reader, and including a defence of the Sunday against the Saturday, which latter day has (I believe) still some advocates in Christendom, as being the Sabbath of the Bible. I was induced to read some modern German publications chiefly on the Theory of Numbers, which is a favourite study of John Graves, though I have very little attended to it. I scarcely knew before I was with him lately that theorems respecting *real integers* have been extended to *imaginary integers*, such as $3 + 7\sqrt{-1}$ under the name of *complex numbers;* and that in this extended view the number 2 (for instance) *ceases to be prime* because it is = the *product* $(1 + \sqrt{-1})(1 - \sqrt{-1})$; though 3 *remains* a *prime* number. Graves pointed out to me that, in a future theory of *integer quaternions no real*

* The following is an extract from the Preface of the *Catalogue of Books in the General Library and in the South Library of University College, London*, 1879:—'The Graves Library is a most valuable collection of more than ten thousand books, and about half as many pamphlets. Mr. J. T. Graves, M.A., who was Professor of Jurisprudence in the College between the years 1838 and 1843, by his will, dated March 26, 1870, devised "all his Mathematical, Astronomical, and Physical books and papers, to University College, London, in remembrance of his former connexion with that institution." Perhaps no private scholar has ever formed a mathematical library so nearly complete. Many of the books are very rare—some probably unique—and about one-half of the whole collection is in handsome bindings.'

integer will continue to be prime; because in quaternions

$$w^2 + x^2 + y^2 + z^2 = (w + ix + jy + kz) \times (w - ix - jy - kz),$$

and *every real and positive integer* is known to be the sum of four square numbers (0 included). I delighted* him by dashing off† a solution of a problem suggested by Scheffler's investigation respecting the "greatest common measure of two complex numbers," which he had supposed would be found difficult, namely, "to find the greatest common measure of two proposed quaternions." He named at random, $1 + 2i + 3j + 4k$, and $5 + 6i + 7j + 8k$; and I soon assigned (by a general process) $i - k$ as their greatest common measure: multiplied (it is understood) by ± 1, or $\pm i$, or $\pm j$, or $\pm k$.

'John Graves has *given* me what it was a wonderful kindness and favour for a book-*collector* to bestow, a duplicate copy of the now rare work of Gauss, *Disquisitiones Arithmeticae*, 1801, which I had the astonishing honesty to *return* (after keeping it only a single year) to Sir John Herschel about ten years ago. It is (I understand) very difficult now to procure it. As some slight return to Graves, I have written to him a long mathematical letter to-day, showing how a few of Gauss's theorems may be extended to quaternions—with suitable but instructive modifications.

'Another German book, besides Scheffler's *Unbestimmte Analytik*, Hanover, 1854, which treats of problems respecting real and complex numbers—founded very much on the great work of Gauss, and containing the solution of the problem of the *greatest common measure* for ordinary imaginary integers, was by the same author, and was among those partly read by me during my recent Cheltenham holidays; namely, his *Situations-Kalkul*. About this book, published in 1851, I had felt a curiosity, because it professes to deal with the *multiplication of lines in space;* and I wished to know whether the author had in any way re-invented the quaternions, or made out anything better, but having the same general aim.

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* In his first rapture he exclaimed 'I see that quaternions will do everything'—you will remember that these were *his* words, *not mine.*

† In a letter to his son, written from Cheltenham, August 21, Hamilton writes, 'I dashed off before breakfast (indeed in my shirt) a *general method* [for its solution], though I had never thought of the problem before; and after breakfast went through the *numerical calculation.*'

You are at liberty to conjecture whether I was more disappointed or pleased by finding that, notwithstanding his imposing symbol

$$x + y\sqrt{-1} + z\sqrt{-1}\sqrt{-1},$$

for a line in space, the *laws* of his multiplication of such lines are precisely those which occurred to me, *in my first crude guess of* 1830, recorded in paragraph [41] of the preface to my lectures, and by me very wisely discarded—though Scheffler has managed to spin a book about them!'

From Sir W. R. Hamilton *to* J. T. Graves, f. r. s.

'Observatory, *September 30th*, 1856.

'. . . And really I should be forced to consult my books for the purpose of even telling you the *titles* of the things I have written upon since we last met, if it were necessary that I should enumerate them.

'For I had almost forgotten, till something led me just now to open the large book to which I allude, that I had written about ten pages in it, during this expiring September of 1856, containing an application of quaternions to a question of solid geometry, suggested to me lately by a recent article of Grassmann's in Crelle's *Journal* [*der Mathematik*]. The article was one of plane geometry alone, but the author was well worthy to have anticipated me in the discovery of the quaternions; and it appears to me a very remarkable circumstance that he did *not*. See my quotation, in page 62 of the Preface to my *Lectures*, from the Preface to his *Ausdehnungslehre*, if you do not happen to have that obscure, but highly original, work in your library. I know that you have the almost equally interesting, and to me far more pleasing book, the *

of Moebius. The last-named writer is a *very* original one, and a *wonderfully* candid one: if you saw his letter of last autumn, in which he *lectured* upon my theory of the circular hodograph, you

* Probably this blank space should be filled by the title of A. F. Moebius's book *Der barycentrische Calcul* (8vo: Leipzig, 1827).

could prove to Mrs. Graves that scientific men are not always jealous of each other.*'

As has been already recorded, it was in letters commencing with the 7th of October that Hamilton communicated to Mr. J. T. Graves his discovery of the Icosian Calculus. The first letter of this series affords interesting proof of his scrupulous care to act with perfect fairness towards a brother mathematician regarding priority in arriving at important conceptions. On this account I here reproduce it. After adverting to his repayment of a trifling loan of money which his prolonged visit to Cheltenham had caused him to seek from his host, he continues :—

From SIR W. R. HAMILTON *to* JOHN T. GRAVES, F.R.S.

'*October* 7, 1856.

'. . . There is a much more interesting, if not more useful sort of honesty, which chiefly urges me to write to you at this moment. . . . I want to know whether you have anticipated me in the finding (for discovery may be too grand a name for it) of a new system of geometrically interpretable, and algebraically non-commutative symbols, quite different from i, j, k. If so, in the parentage of thought these new symbols, ι, κ, λ, will be bound to pay a certain filial duty to your brain. For it has just occurred to me to remember that you spoke in the Mathematical Section lately of a new imaginary, the nature of which you did not communicate, suggested to you by considerations of polyhedra. Now it was precisely that *class* of considerations which led me lately to my own result. That *they* (the polyhedra) *interested me*, is chiefly due to you ; but I am not *conscious* of your unexplained remark about a new *imaginary* having *influenced* my recent speculations ; nor, indeed, should I quite like to call my new symbols "imaginary." If you choose *now* to tell me what *your* result was, I shall not be unwilling to receive the information, since I have something to offer

† In a letter dated Leipzig, August 27, 1855, Moebius writes, 'In my lectures and otherwise I have tried to divulge the most elegant and simple way, by which in the last-named Paper you derive the Keplerian laws from Newton's law of gravitation.' Manuscript Book L. 1847, p. 156.

in return. On my part, I am about to enclose a *memorandum* of the nature of my new system, which I shall seal up in a separate cover, so as to give you the *option* of not reading it, should it be any satisfaction to you not to do so till after your writing to me. But of course I have not the least *objection* to your opening the enclosed packet at once. . . .' *

From the SAME *to the* SAME.

'OBSERVATORY, *October* 17, 1856.

'MY DEAR JOHN GRAVES—The constant friendship to which you have admitted me, during a period of more than thirty years, may cause you to remember, with some interest, that you were the first

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* The enclosed *memorandum* states with such conciseness the substance of the discovery that I insert it here, supposing that it may be welcome to some mathematical reader.

MEMORANDUM for *John T. Graves, Esq.*, from his friend, *the Author.*

'It has lately occurred to me that several investigations respecting polyhedra may be assisted by the introduction of a system of new symbols, ι, κ, λ, which shall be symbolical roots of positive unity, but uncommutative (although associative) as factors among themselves. For example, I find that if we assume

$$\iota^2 = 1, \quad \kappa^3 = 1, \quad \lambda^5 = 1, \quad \lambda = \iota\kappa; \tag{A}$$

then the only *cyclical* mode of passing over all the successive faces of the icosahedron, or (along 20 of the 30 edges) from corner to corner of the dodecahedron, is fully expressed by the formula

$$k_1^3 \, k_2^3 \, (k_1 \, k_2)^2 = 1, \tag{B}$$

where $k_1 = \iota\kappa, \quad k_2 = \iota\kappa^2$. (C)

By making, similarly,

$$l_1 = \iota\lambda, \quad l_2 = \iota\lambda^2, \quad l_3 = \iota\lambda^3, \quad l_4 = \iota\lambda^4; \tag{D}$$

so that $k_1 = \lambda$, and $l = \kappa$; (E)

while k_2 and l_2 l_3 l_4 have the significations stated above, I find that all results respecting passages over *successive faces* of the dodecahedron, or along successive edges of the icosahedron, so as to cover *all*, or *some*, of the faces of the one solid, or to pass over *all* or *some* of the corners of the other, admit of being expressed by formulæ of the same general kind as (B), with l's instead of k's for factors. For other factors I use other exponents.

'W. R. H.

'OBSERVATORY OF T. C. D.,
'*October* 7, 1856.'

person to whom I communicated my invention of the Quaternions, in a letter of October 17, 1843, which has since, by your permission, been printed.*

'I had some hope that you might have anticipated me in a more recent mathematical conception, the result of a train of thought for the suggestion of which I am indebted to yourself. With your usual candour you have informed me that you have no claim to make in the matter. Let me at least have the pleasure of in some degree associating your name therewith if this letter shall be thought worth preserving; for I feel sure that if you had not lately pressed on my attention the geometrical interest of the polyhedra, although the feeling of such an interest is among my very earliest mathematical recollections, I should not have been conducted to that novel† system of symbols, respecting which I have now the pleasure of giving you some enlarged particulars, without pretending to attach more than a very moderate degree of importance to the results. . . . [Here follows a full statement extending to twenty-three folio pages of manuscript.]'

Subsequently, in a letter which I now insert, Hamilton refers to the investigations of the Rev. T. P. Kirkman on polyhedra.

From the SAME *to the* SAME.

[FROM A COPY.]

'OBSERVATORY, *November* 1, 1856.

'I am much obliged to you for drawing my attention to Mr. Kirkman's researches on Polyedra—you see that I consent to drop the "h."

'Though I admire his inventive powers, I happened to be entirely unacquainted with what he had done on the subject referred to. I suppose that I *must* have just *seen* his name printed in connexion with polyedra; but if I did so—in the notices of the *Proceedings* of the Royal Society, for instance—it made no impression on me whatever, because I had no thought of pursuing the subject

* See *supra*, vol. ii., p. 463.

† 'Of course when I say "novel," I can only mean that it is such to myself at present.'

myself, and did not hope to understand the abridged account, without having the fuller statement before me.

'It was only yesterday that I observed that the First Part of the *Philosophical Transactions* for 1856 had arrived here, and that I began to cut the leaves. After a fashion, I have read the two papers of Kirkman at the end; enough to catch what he is at, and to see that on the geometrical side he has been dealing with far greater generalities than myself. But I do not observe the slightest trace of his having caught even the notion of my algorithm or *calculus*, such as it is; and my long letter to you contains only *specimens* of such a calculus. Even as regards my "q—agon," namely, a certain 20—agon, composed of 20 successive edges, out of the 30, of the ancient dodecaedron, I do not see that Kirkman has *printed* an anticipation; though I am most willing to believe that he *possessed* one, and thought it too easy to publish.

'The conditions of his Theorem A, page 413 of the Part [of the *Philosophical Transactions*] above cited, do not seem to *include* my result; for though every "crown-summit" of the old dodecaedron adjoins one of the "wall-summits," it is not true, conversely, that every wall-summit is contiguous to a *crown* one. But Kirkman, in page 418, expresses himself as aware that a "q—agon" can be traced along selected edges of a "q—acron," *without* all the conditions of his Theorem A being fulfilled. Still, I do not see that he has *specially* perceived the closed polygon which winds round all the twenty corners of the Dodecaedron of Euclid and Plato.'*

From JOHN T. GRAVES *to* SIR W. R. HAMILTON.

'CHELTENHAM, *November* 13, 1856.

'. . . It is pleasant to me to find that you have not been at all anticipated by Kirkman, and the conception of your [Icosian] calculus strikes me as not only original, but likely to lead to a good deal.'

* 'Plato, in the part of the *Timæus* to which I lately referred, seems to speak of the Dodecaedron, or rather to hint at it, as the most mysterious of the solids, and as perhaps typifying some other world to us unknown—a sort of *quintessential element*. At least such is my impression, unassisted by the writings of any commentator.'

CHAPTER XLII.

'LETTERS ON QUATERNIONS.' AUBREY DE VERE'S 'MAY CAROLS.' BRITISH ASSOCIATION IN DUBLIN. CORRESPONDENCE WITH SALMON.

(1857.)

ONE of Hamilton's neighbours living at Farmleigh, a country-house adjacent to the Phœnix Park, was Mrs. Smythe, an English lady of much intelligence and culture. His visits to her house were often sought for, and to congenial guests there assembled he not unfrequently gave delight by reciting or reading to them poems, not confined to his own and his sister's (though these were in request), and by imparting to them information on various sciences, and discussing with them questions on connected difficulties. Linnæus and Botany, the Bible and Geology, Burns and Tennyson, I find by his note-books were subjects so discussed, and I give at foot in substance two of these memoranda, which are not without interest.*

But I here mention this lady because, by seeking from Hamilton on behalf of a sister (Mrs. Frederick White) whose tastes were scientific, a general notion of Quaternions at the same time that Dr. Nichol of Glasgow was urging him to contribute an article on the subject to his *Cyclopædia of Science*, she was the cause of this

* 'Is it not Bacon who says that truth will more easily emerge from error than from confusion? Linnæus, it seems to me, considered himself as acting on that view, and regarded his confessedly artificial system as only the pioneer to some *more* natural system of Botany (for Linnæus's system was *very* natural in many respects) such as that of Jussieu has been accounted to be, and which in its turn may, and probably will, merge in some still more natural system. If I even dimly remember the Latin words of Linnæus as printed in a delightful little book not very generally known even to botanists, the great Swedish

article taking the form of three *Letters on Quaternions*, the first of which was addressed to "Mrs. S——." He felt, however, that what he had to write in addition to the contents of that first letter could not with probability be supposed to be intelligible to a lady who was no more than a dilettante in science, and on this account the second and third letters were addressed to an imaginary gentleman. They give a very full view of the nature and modes of application of the Quaternion Calculus, and of his own work upon it up to the time at which they were written: and in them, as in all his writings, he takes care to assign to other mathematicians engaged in the same field of research their due amount of credit. The conclusion of the third letter gives an interesting statement of his views and feelings with regard to his own relation to his Calculus, and of his expectations as to its future. His estimate of what he had accomplished in the use of it as an instrument is most modest: the last few sentences of the letter may here be reproduced:

'Observatory of Trinity College, Dublin,
January 24, 1857.

'. . . The quaternions seem to me to admit of entering into an alliance so close, yet new, with *every part* of pure and applied *geometry*, and at the same time to require such *large* additional developments, before their relations of analogy and contrast to existing methods of *calculation* shall be fully known, that I count myself *merely* to have *begun* them. The field is far too wide to be tilled by a solitary labourer, even with occasional assistance from a few friends,

naturalist did say "*Primum* atque *ultimum* in Botanicâ methodus naturalis debet esse."

'I suppose that we must admit some symbolical element into the interpretation of the First Chapter of Genesis. At least the obvious and literal interpretation of it appears to be no more consistent with known facts of science than those passages which speak of the Earth as being at rest, &c., in other parts of the Bible . . . What right or reason have we to imagine that a *scientific* revelation was entrusted to Moses? and for the Jews! those children to whom the Law was as a schoolmaster; though I know that to them were committed in a most high and sacred sense the "Oracles of God."'

who feel some interest in his exertions. The time may come, though, if so, it will be due to other explorers rather than to me, when, the *mathematics* of this calculus having become comparatively mature, it shall admit of being extensively and usefully *applied* to *physics*, as a new *instrument* in the study of *Nature*. In the prospect of such a time, I feel with no jealous pain, that although it may have been permitted to me to accomplish *something* in this enterprise as an honourable Suitor of Science, yet the Bow awaits its Ulysses.'

Early in the summer of 1857 an additional and striking proof was afforded of Hamilton's scrupulous care to guard the rights of others as well as his own in scientific discovery. His friend Mr. John Graves had sent him from Cheltenham, for communication to the Royal Irish Academy, a Paper entitled '*A Fundamental Theorem respecting Congruences affecting a Class of Complex Integers which involve the Imaginary Cube Roots of Unity*': having received permission from the Council, Hamilton had given formal notice of the reading of this Paper, carefully assigning the authorship to Mr. Graves: but by some mistake this notice, when printed, omitted the clause naming the author, and thus the Paper appeared to be a contribution from Hamilton himself. He instantly, at his own expense, had 500 copies of a memorandum correcting the mistake printed for distribution among the members of the Academy: he did this, not thinking it right to wait for the next Academy meeting, at which in point of fact, he read the Paper, in conjunction with two of his own, but taking particular care to indicate the distinct authorship. It may not be unnecessary to point to the fact that the Paper was one of which he was willing to be the sponsor, and the results of which he employed himself in extending by means of Quaternions. [See MS. Book, 1856. 4to.]

As another indication of character, I insert a note written in the summer of this year to the same old friend. The foible of vanity was one from which he felt that he was not exempt. In this note he ingenuously betrays his consciousness of the infirmity, the evil of which he seeks not to palliate; and he takes a legiti-

mate pleasure in what he rightly judged to be a proof that it did not deeply penetrate his nature. The letter from Mr. J. T. Graves, to which this was a reply, had brought to his knowledge the glowing recognition of the discovery of Quaternions, recently published in the *North American Review*,* and at the same time had made mention of the illness of the writer's sister, Madame Ranke.

'Observatory, *July 26th*, 1857.

'My dear John Graves—I remember reading, long ago, in Coleridge's *Biographia Literaria*, or in some other work of his, a passage which ran somewhat thus:—"For the passion of Vanity is a loveless passion—loud on the hustings, gay in the ball-room, mute and sullen by the family fireside." I hope that it may be accepted as some indication of what, I trust, is the fact, that my moral nature has not been hopelessly corroded by vanity, when I tell you that your slight mention (though made with all due fraternal affection, and, therefore, *not* deliberately to be called *slight*) of your sister so entirely obliterated all recollection of the compliment which you say that I have been lately paid in America about the Quaternions, that I had absolutely *forgotten* all that part of your recent letter when I was lately writing to you. Yet I remember, with great interest, that my scientific labours were very early noticed in America. But, somehow, I look quite *coldly* now on compliments of all sorts, though certainly not less *genial* in temperament, nor more *indolent* in spirit and exertion, than I was long ago. Here is a whole basket of egotism to prove that I am *not* egotistic! which, after all, I *am*, but still not so much as to obscure some common sense, nor to deaden any affection.—Affectionately *yours*, at all events,

'W. R. H.'

The following portion of the correspondence with Mr. De Vere will be welcome to the reader, as casting additional light upon the mutual relations of the two friends:—

* *Supra*, vol. ii., p. 455.

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'7 PARK-STREET, WESTMINSTER, *July* 29, 1857.

'On my arrival here, a few days since, I was greeted by a letter from you a little more than a year old! It was dated July 18th, 1856, and related in a large part to Mr. Penny, for your kindness to whom pray accept my best thanks. , . . I only wish I had had an opportunity of making you and Dr. Newman mutually acquainted. However much you may differ on some subjects, there are a hundred which would alike interest you both; and that two such persons should have been so long near each other, without even meeting, is a piece of *bizarre* irony on the part of that Social Fate which holds us in her iron meshes. But you both seemed full of occupations and engagements, and probably had no time for new ones. He is now at Birmingham.

'I have had a charming winter and spring at Rome. All the days of both seasons passed in a satisfactory way, except one sad fortnight spent in the dying room of my poor friend, Robert Wilberforce (late Archdeacon Wilberforce). It really seems as if Rome were inexhaustible. Every year one has to begin again, and open on a new world of interest. On my way back I visited several marvellous old Etruscan cities, in the neighbourhood of which are tombs 3000 years old, stored with all manner of things, from monuments to ear-rings, as fresh as the day they were made. At Venice I passed three weeks, sailing about the water-streets of that Queen of Cities—the Cleopatra of the waves, and the most beautiful of earthly pageants. On my way home I sojourned for a few weeks beside the Italian lakes, and under the shadow of Mont Blanc; and now here I am again in Babylon.

'I do not know whether you have or have not received a copy of a new volume of poetry by me, called *May Carols;* but I am pretty sure I directed Longman to send you one. It is an attempt, like my hymn on the *Feast of St. Peter's Chair at Rome*, to put theology into verse; but, to make it less dry, I have broken the poem into a number of short pieces, thus making it a *serial* poem, such as were common in the Elizabethan age, as well as in Italian literature—a form of composition revived in our time by Wordsworth (*Sonnets to Liberty*), and Tennyson (*In Memoriam*), and others. I have endeavoured in each piece to indicate, as briefly as

possible and in mere outline, *some one leading idea* or *principle* bearing on the Blessed Virgin, and therefore illustrating that vast and wondrous doctrine—the Incarnation—which in these days is so little really taken in. The position assigned to Mary in the Scheme of Redemption is a theology in itself; and, without mastering it, I own it seems to me most difficult to master with the mind, as well as realize with the heart, the Divine Economy of the Incarnation, and of Mediation. I have not, you will observe, shrunk from making statements as strong, *doctrinally* (apart from exaggerated or rhetorical *language*), as have ever been made; but, on the other hand, I have endeavoured, in other pieces, to affirm with equal plainness those other great, and, as I think, correlative Christian Doctrines, which, in the eyes of many Protestants, *appear* incompatible with the assigning of so exalted a place to Mary. In other words, I have endeavoured not to extenuate the "Cultus Deiparæ" and present it in a flattering form, but to illustrate it at both sides; not drawing a "Via Media" line, but drawing a circle large enough to include the converse Truths (the one set belonging to pure Theism, and the other to the Incarnation), which must be reconciled in the theology of the finite *creature* elected to be the Mother, in His Humanity, of her Infinite Creator. I do not know whether the book is at all calculated to *recommend* the Roman Catholic teaching on this subject; but I should think it may help to make it more *intelligible* to minds willing to regard the subject with candour, and with the attention required by a subject at once so large and so subtle as Theology. At the same time, I should think it not unlikely the poem may somewhat frighten those who have been in the habit of looking at the subject only from one point of view, and with the associations that have gathered round the Protestant tradition. You must tell me candidly what you think of it, both philosophically and as a literary experiment. I send you a copy. If you have got one already, pray transfer it, with my compliments, to your friend Mrs. Wilde.'

From Sir W. R. Hamilton *to* Aubrey De Vere.

'Observatory, *July* 30, 1857.

'My dear Aubrey De Vere—We have not written to each other for a long time—at least a year; but you have often said,

what I always recognised to be the truth, that you and I, on every occasion of fresh writing, *went on* precisely as if there had been no gap in our correspondence. In fact my love to you has never been for a moment impaired—nor my respect; although I am farther than ever from participating in your Romeward movement: I do not call it *change;* for, on looking back on those old days, when we seemed so much to agree, not only in sentiment, but also in belief, on almost every subject, I can see that, while you were (in heart—and I do not mean in disguise) a Catholic *aiming* to be a Protestant, because your father was so, I was throughout *essentially* a Protestant, though with many Catholic sympathies. I request you to believe that I do not belong to the class of irreverent scoffers at Popery.'

From SIR W. R. HAMILTON *to* MRS. WILDE.

'OBSERVATORY, *April* 13, 1858.

'Can I better, or more pleasantly, commemorate our first meeting, which occurred exactly three years ago, on the 13th of April, 1855, at the hospitable house of Colonel and Mrs. Larcom, than by introducing to you my old and dear (I regret that I am obliged to add my Popish) friend, Aubrey De Vere, the poet and prose-writer, with some of whose principal works in prose you are already acquainted?

'Some faint remonstrance notwithstanding, the modern Romanism, of which Aubrey is a splendid type, tends to the introduction of a Tetrad into the human notion or belief of the Divine Nature or Essence.

'I cannot believe that this result, or even this tendency, is at all short of idolatry.

'It is idle to talk of *Images* exposed, and falsely pretended to talk or wink: the *Poems* of Aubrey De Vere sufficiently exhibit the essential idolatry (for the word "Mariolatry," though just, may be dispensed with as unnecessary) of the Church which he has adopted.

'I did not intend to say so much: in general, I care very little whether an acquaintance of mine is of this or that religion; and, in the present case, I retain a very deep affection for the individual author and friend of whom I have been writing.

'He will never be a Protestant again; or, to speak more

accurately, I venture to pronounce, from what I knew of his boyhood, that he *never was* a Protestant: although, of course, we are bound to believe that he *thought himself* to be such, when he wrote that grand essay on *English Misrule and Irish Misdeeds*, of which you lately accepted the loan, and which you joined me in admiring.'

The last week of August, in 1857, brought to Dublin the members of the British Association for the Advancement of Science for their second meeting in the Irish metropolis. It will be remembered how distinguished a part was taken by Hamilton at the first meeting in 1835, when he acted as joint-secretary with Professor Lloyd, under the Presidency of the venerable Provost, Dr. Bartholomew Lloyd. Professor Lloyd had now become Dr. Humphrey Lloyd, one of the Senior Fellows of Trinity College, and had been chosen to be President for the year. Lloyd's expressed wish had been that this honour should be conferred on Hamilton; but it was thought that his own claim was one which ought not to be set aside, and the reader will recall that at Cheltenham, at the meeting of the previous year, Hamilton had seconded the proposal that his friend should be appointed to the office. As a Vice-President, Hamilton was a regular attendant at the meetings, and he made a communication to the Mathematical Section, *On some Applications of Quaternions to Cones of the Third Degree.* This application of his Calculus to Surfaces of the Third Degree was considered by Hamilton to be in the working of Quaternions a step forward of very considerable importance. This is proved by a letter written on the 14th of April in this year to Dr. Lloyd, which for that reason I here insert:—

'One thing that has delayed me has been, as you may be glad to know, my having had occasion to write to Mr. Salmon, to announce to him a totally new application of the Quaternions, namely, one to Cones (and in fact to Surfaces) of the *Third Order.* Strong as my *à priori* confidence in the Quaternions has been for many years, on account of the laborious investigation to which I have long submitted their *principles*, I still have had a misgiving,

lest their *practical usefulness* might be bounded within the (not narrow) limits of the theory of Surfaces of the *Second Order*. At all events, I was *content* for many years to *limit myself* to the study of *such* Surfaces, as objects for the application of the Quaternions. But it occurred to me, as I was walking up through the fields last night, after attending a meeting of the Royal Irish Academy, that I could *represent* the *General Cone of the Third Degree* by the very simple binomial equation, interpreted on the principles of my Calculus,

$$S q \rho q' \rho q'' \rho = 0 ; \qquad (A)$$

where ρ is the *variable vector*, or side of the cone, and q, q', q'' are *three arbitrary but constant quaternions*. *Three important planes, analogous* to the *two* real *cyclic planes* of a cone of the *second* order, are thus at once suggested to me, namely, those denoted by the three following equations.

$$S q \rho = 0, \; S q' \rho = 0, \; S q'' \rho = 0. \qquad (B)$$

And a fourth plane (C), which is a sort of *transversal* to these three, has been obtained by me this morning through calculations sufficiently easy; but which plane, with the former, seems likely to play an important part hereafter. I am ashamed to say that I have never read Newton's *Enumeratio*, &c. Now that I have caught hold of a single formula (A), which *must include* all old results of that sort, I long to sit at his feet. . . .'*

The *Athenæum* (September 12, 1857, p. 1148), recording the above communication to the Association, adds :—

'It would be impossible to give the general reader any clear idea of this abstruse paper; but at its conclusion the soundness of the principles on which the author proceeded was made strikingly manifest to the Section by Mr. Henry J. Smith of Oxford, explaining in fully as lucid a manner as that of Sir W. Hamilton (who makes everyone that hears him for the moment think that he clearly comprehends the whole subject), how by the method of Quaternions, but by a different process from that of Sir W. Hamilton, he had in some of the examples selected by Sir W. Hamilton, arrived at precisely the same numerical results.'

* Compare *Elements of Quaternions*, 1866., p. 705.

By a letter to Mr. Salmon, of August 30, 1857, given further on, it will be seen that Hamilton, in his characteristic way, free from all jealousy and welcoming co-operation, was anxious to secure for Henry Smith an opportunity of communicating his results in full to the meeting. The impression made by this excellent mathematician, accomplished scholar, and public-spirited citizen upon his countrymen (for though an alumnus of Oxford he was a native of Ireland) was in all respects at one with that which, wherever he was known, made him the delight and admiration of his friends. The intercourse between him and Hamilton did not cease with this meeting; I find that in after days letters passed between them.

Hamilton also expounded to the Mathematical Section his Icosian Calculus, and distributed a lithographed illustration of the Icosian Game. And he made an oral communication in connexion with Quaternions, for which I find some incomplete preparatory notes, which seem to me to contain points of importance in the history of the development of that Calculus: I therefore insert them here.

'*August 29th*, 1857.... As I shall have occasion in my communication of this morning to introduce a few remarks on geometrical and symbolical imaginaries, I may say here that I have long ceased to *call* my symbols i, j, k, *imaginaries*. They are indeed *square roots of negative unity*, and are so far analogous, in calculation, to the old $\sqrt{-1}$. But besides those other differences, even in *calculation*, on which I have often remarked, and especially those which arise from my *non-commutative* law of multiplication, when I came to see how very easily and simply the i, j, k, may be *constructed* in geometry, as denoting a system of *three rectangular unit lines*, I soon abandoned the *name* imaginary, as relative to i, j, k; and I felt, for some time, a hope that we might thus come to get rid entirely of *imaginaries*, and might retain only *reals*, in calculations thus applied to *geometry*. But just as in many well-known researches of modern geometry—I need scarcely mention the celebrated writings of Chasles—nor need I speak of the works of our own Salmon—*imaginary points*, lines, and surfaces,

are boldly and copiously and usefully employed; so in the prosecution of *quaternions*, and in their continued application to *geometry*, I found it absolutely *necessary* to admit *geometrical imaginaries*, and to represent them by *symbols*, in which the *old* and *ordinary* $\sqrt{-1}$ was *combined* with my own i, j, k, and which I called *bivectors* and *biquaternions*.'

On the last day of the Association week, although he had to attend in the morning the funeral of a relation, he made a special effort to be present at the final meeting, in order that he might manifest to the end his loyal homage to the Presidency of his friend. For this loyalty he could not have received a more gratifying reward than was conveyed in the earnest thanks expressed to him at the time by the wife of the President; and in the letter, written by her shortly afterwards, which, without asking her permission, I venture here to insert. No one who has known the writer—what she is in herself, and what she was to her husband—will be surprised at its high feeling and noble expression. Hamilton, with his habitual sense of justice, forwarded the letter to Mrs. Robinson, in order that her husband might enjoy his coequal share in this generous recognition of generous feelings.

From MRS. HUMPHREY LLOYD *to* SIR W. R. HAMILTON.

'KILCRONEY, *September* 15, 1857.

'MY DEAR SIR WILLIAM—Having at last an hour of leisure, after the many remnants of business which remained to be arranged previous to our leaving home for a few weeks, I wish to send you a few lines, that you should no longer marvel at the warmth with which I spoke on the day of the closing meeting: as indeed I do not wonder that you should have hitherto done, for I dare say I expressed myself with an energy which is very unusual with me. I was, at the moment, under the influence of many feelings and recollections which very deeply moved me; and in the words I uttered were in fact contained the thoughts which reached back to former times and to other persons as well as yourself. I had seen on that day, and throughout the meeting, Dr. Robinson and your-

self (many others I might also name) rejoicing at, and assisting in, all that was to bring honour to Humphrey. I knew well that this cordial feeling, however *generous* it might be, was nothing uncommon, but that it had always been thus; and knowing as I do how rare is such feeling—how little the most exalted intellectual merit exempts from a lurking jealousy, when the claims of another are in any degree brought forward—I thought it was no less beautiful than to me most delightful to mark the warm appreciation of my husband's character, moral and intellectual, which you then and always seem to take pleasure in showing. I felt, too, how very gratifying it is that he has been so little entangled in any angry strife with his brethren, and how precious was the kindly sympathy of those amongst them who had known him best and longest. Thus you will perceive that my words to you were the expression of a complicated emotion, concentrated upon you as the representative of so much which called for my gratitude. I am writing in haste, for I wish to send these few lines before I leave home; but I think you will not be at a loss to understand what I mean to convey, nor will you deem the feelings which influenced me either misplaced or mistaken. Believe me sincerely yours,

'Dora Lloyd.'

Early in the May of 1857 Hamilton entered upon a correspondence on Quaternions with Dr. Salmon, at that time Donegal Lecturer in Mathematics in the University of Dublin, and no less eminent then for his mathematical than he has since become, as Regius Professor of Divinity, for his theological works. In this correspondence, which was carried on to the end of September, Dr. Salmon places himself as a learner at the feet of Hamilton, states frankly his difficulties and objections, gradually overcomes his original repugnance to the revolutionary character of some of the processes, and while retaining a doubt whether a large part of the working equations in Quaternions might not admit of being conveniently translated out of the quaternion notation, concludes by saying (August 26th, 1857) that, 'even if this were so, the admirable consistency and harmony of the whole scheme is deserving of all praise.'

In reference to these words of Dr. Salmon, it may be here suggested that the subsequent history of Quaternions has established, at least in certain departments of research, the practical advantages of the Calculus, as well as its theoretical perfection; indeed, this result is anticipated by Dr. Salmon himself; for an earlier letter of his (June 24th, 1857) closes with the following sentence:—'I hope you will not be lazy about the Covariants and Contravariants. I have no doubt that great fruits will come from the marriage of the Quaternions with the Calculus of Forms, and no one but you ought to give away the bride.'

This correspondence between Salmon and Hamilton is one which would necessarily prove of the highest interest to students of Quaternions, who would find in it the real difficulties of the Calculus urged by the ablest of learners, and solved by a teacher of all others the most competent. It is to be hoped, therefore, that in due time it may be transferred to print. I make here a few extracts touching points of interest, from letters of Hamilton to Dr. Salmon and Dr. Ingram.

From LETTERS *to* DR. SALMON.

[A. 1857, p. 415]—*June* 23, 1857.—'. . . It is fair to say that (when too late) I found that Grassmann had independently (but perhaps not quite so soon—still it is not a matter worth contesting) arrived at the same conception and notation, respecting the DIFFERENCE OF TWO POINTS (B – A), regarded as their *directed distance*—what he calls "strecke," and I "vector." But this is merely a *preparation for quaternions*, and not as yet *in any degree* the Doctrine of the Quaternions *themselves*.

'I admire Moebius very much indeed, but he has (I think in his *Barycentric Calculus*, &c.) approached *less nearly* to the quaternions than Grassmann in his *Ausdehnungslehre*.'

'*July* 25, 1857.—No copy was preserved of the note, and indeed I hope that nobody will be so unkind to my memory as to dream of printing *all* the (perhaps many thousands of) notes or letters which I have written in the course of my life; though I

am not *quite* so modest as not to fancy that a judicious and *sparing* —or, if you like the word better, *un*sparing—*selection* might yield something which "the world would not willingly let die." In short, destroy at least 90 per cent., and the last tenth might be perhaps preserved.'

[A. 1857, p. 481].—'P. S. I wonder whether Cayley (or indeed *any one else*) is *as yet* able to read my Appendix C. At this moment I do *not* believe it.

'I see that my P. S. has not been a particularly *modest* one; but can assure you that Cayley has written heaps of things, which *I* could not without deep study read.'

[A. 1857, p. 483]. *July* 27, 1857.—'. . . Observe for a moment the vast range of the *aim* of what my brother astronomer Airy, not usually supposed to be a flatterer, was pleased to call, on receiving my book, the "large science of Quaternions." The question is not what Hamilton may be *able to make* of it, but what his *conception* of the matter is, and what he hopes that *others* will yet accomplish. And the AIM is no less than this: *to impregnate all existing algebra* (including the Differential and Integral Calculus, Calculus of Variations, &c.), *and all pure and applied geometry, with a* NEW ELEMENT *of thought and calculation* (through i, j, k, &c.). *Algebra* (including the covariants) becomes thus in *thought*—I am very far from saying in *achievement*—that particular CASE of the quaternions for which the *coefficients* of i, j, k, are *zero*. But you are well aware that the *memory* must be *trained*, in any department of study whatever, before results and rules can become *ready* to be applied. Still I do not despair of *classifying* the *chief* rules and results which I myself habitually, and almost unconsciously, employ (all *ultimately* derived from the laws of i, j, k, or from the formula, $i^2 = j^2 = k^2 = ijk = -1$) so as to *assist* the *memory* of a student—and even to draw up some very SHORT manual, which might not task too much the patience of an *undergraduate*. At present I know that my book is quite unfit to enter into the undergraduate course.'

[A. 1857, p. 515]. *August* 14, 1857.—'Meanwhile I am charmed by your saying that *if* your result does not agree with mine, so much the *worse* for *me!*

'*That* is the true and rational footing to put the thing on; and far more really flattering, or pleasing to me, than if a person were to say—as there is a story about a courtier saying to Marie Antoinette, in those days when (Edmund Burke) "a thousand swords would have leaped from their scabbards" to avenge the slightest insult to her, and when she is reported to have inquired what o'clock was it? "Whatever your Majesty pleases!"

'Mine is a constitutional government—and more, in it the king *can* do wrong. I admit this the more cheerfully, because *as yet* I am not aware that you have caught me tripping. But you *may* do so; and I trust that when the time comes you will find my good humour invincible.

'Go on, by all means, and don't fancy that I want to teach people conics instead of quaternions. I only thought that *cones* might be selected as a sort of *basis* of illustrations.'

From LETTER *to* DR. INGRAM.

[A. 1857, p. 517]. *August 14th*, 1857.—'Salmon is getting on so awfully fast in the Quaternions, that if I don't take care we shall get into some contest of priority!

'In a note received from him this morning he says—at the end of some railway pencilling—"I have not got your Paper to see whether this agrees with your result, but if it does not, so much the worse for you!" You conceive, of course, that this is just the thing I like. I am quite tired of being a Fee-faw-fum, in Quaternions, or in anything else. The highest reward that can be given me for my labours in that, or in any other department of science, is to take it out of my own hands.

'As regards my old researches in Physical Astronomy, such a result has already taken place, to my great satisfaction. Jacobi, very early, did me the honour to take them up as a commentator: and, of course, as an enlarger—nil fere non tetigit; nil tetigit quod non ornavit. More latterly, Dr. Houel, whom the Committee of the British Association intend to invite to Dublin, has published two Theses (running to a large number of quarto pages) in Paris, for his *degree* as "Docteur es Sciences," on Mechanics and on Astronomy, almost entirely relating to what he is pleased to call my "discoveries." In a lithograph dated Caen, November

8th, 1856, and entitled "Note sur le Théorème d' Hamilton et de Jacobi, et sur son application a la théorie des perturbations planétaires," Houel says: "Mon but, en rédigeant cette Note, a été de faire voir combien, de toutes les méthodes qui ont été proposées pour arriver aux équations de la variation des constantes arbitraires, celle que Jacobi a déduite des découvertes d' Hamilton est la plus directe et la plus simple."

'It is a genuine pleasure to me to believe that in Salmon I shall have a worthy successor, as regards my more purely mathematical researches, and may be much excel, even in quaternions, myself! Meanwhile I am,' &c.

'P. S.—Mac Cullagh once said to me, in allusion to you, that it was no wonder that J. K. I. should have a friendly feeling towards *i j k*.'

P. S. OF A LETTER *to* DR. SALMON.

[A. 1857, p. 518]. *August 15th*, 1857.—'By this time you are prepared to admit, I think, these three things at least about the *calculus* of Quaternions:—

'I. that it is not in any slavish way dependent on co-ordinates;

'II. that it is remarkably rich in transformations; and,

'III. that it is no *mere* calculus of rotations;

but involves a reference to *lengths*, *translations*, and (in general) to *extensions*, as *essentially*, though perhaps not quite so characteristically, as to *directions*.

'Your question about the two meanings of *i* is one of extreme importance. It is THE difficulty in the theory of the geometrical *interpretation* of Quaternions: but you conceive that I *cannot give up* the result; though I *must explain* it further.'

[A. 1857, p. 533]. *September 15th*, 1857.—'I hope you do not think me so *foolish* as to imagine that quaternions will *ever supersede determinants*. All that I pretend is, that the two things are *not identical*; and that on occasions (which I may, perhaps igno-

rantly, flatter myself by thinking to be numerous) the quaternion may have some advantage.

'As you seem to have withdrawn, for the present, from the subject, let me at least ask you to allow me to thank you for the kind attention which you have lately given to it.

'And do not imagine it to be flattery, if I add that I consider you to be *very likely to excel myself*, even in *this*, which (of late years) may be regarded as my *own* department, if you shall ever be induced to pursue it. "To him that hath shall be given." You possess *a far larger stock of geometrical knowledge*, although I have always had something of a geometrical *taste*, and carried away all the honors of my division in geometry, when I was, very long ago, an undergraduate in our University. If you shall ever seriously take up the Quaternions, your *geometry* will enable you to go far beyond *me* in that subject.'

P. S.—*September 22nd*, 1857.—'This note has been laid aside somewhere for a week, and during the interval I have gone through a frightful amount of algebraical and arithmetical calculation, on the subject of a certain class of definite integrals which I do not pretend to have connected with Quaternions. So much the better! You or any other friend of mine could not desire me to become the *slave* of any genie of the ring, or lamp, which may have once answered to my bidding.

'In this instance I am falling back on a research begun as long ago as 1839. In my Paper on "Fluctuating Functions," published in our Irish *Transactions* [vol. xix., part II., p. 313.]—which Paper interested Sir John Herschel, and was partly commented on by G. Stokes—I gave a *law* for the terms of a *descending series* designed to represent the integrals

$$\frac{2}{\pi}\int_0^{\frac{\pi}{2}} d\omega \cos(2t\cos\omega)$$

(I quote from memory), where t is positive and large. A hint from Poisson is acknowledged. Poisson had given, according to my recollection, *a term or two* of the descending series, but *not the law*, which is by no means obvious.

[A. 1857, pp. 536, 537]. *September 25th*, 1857.—'Call them

[Quaternions] a method of polar co-ordinates, by all means, if you choose. I am ashamed of having been so stiff upon that point. Of course you know all about *lengths* entering. I only doubted whether I had made it prominent enough.'

[A. 1857, p. 530.] *August* 30*th*, 1857.—'I thought your transformations very skilful, and my IMPRESSION is, that some (indeed all) of your *forms* for the single-sheeted hyperboloid were *new to me* (your Paper is not beside me at this moment). But you will not think me *greedy of priority*, if I should happen to *meet* some of them in unprinted books of mine, and if I shall venture to *tell* you so. In point of fact I am only *too careless* about priority: and yesterday, for instance, I was quite willing to admit to Hart that whatever Mr. H[enry] Smith's formula for the number of *cyclic* (or, as he called them, *circular*) planes of a cone of the *nth order* might have been, it was probably one which I had not anticipated. But within a few minutes afterwards I recollected that I had found that a *cubic cone* had *fifteen cyclic* planes; and of course that I *must* have used the general formula $n(2n-1)$, which I suppose was what Mr. Smith had stated in the Section. We ought to get him an opportunity for coming forward with a Paper of his own.'

An extract from a letter, written this autumn to John Nichol, brings to light an old criticism of Lord Brougham which confirms the opinion that, great as were the intellectual powers of the versatile Chancellor, he was deficient in higher imaginativeness, and also in accuracy as to facts. It may be balanced, however, by a few words written by Hamilton, concerning the great speech which Lord Brougham delivered in 1858, at the unveiling of Newton's statue at Grantham—that 'it was worthy of the occasion.' In the erection of this statue in the native place of the great mathematician, Hamilton had taken zealous interest, making endeavours, though with little success, to enlist Irish subscribers to the fund raised for the purpose.

From SIR W. R. HAMILTON *to* JOHN NICHOL.

'OBSERVATORY, NEAR DUBLIN, *October* 31, 1857.

'From childhood I have been accustomed to think of Holy Eve (or All Hallows' E'en) as, to me, a *poetical day*, from feeling and association; and I give you my word that I have not written a single *xyz*, nor even a poor *ijk*, since I awoke this morning; although I plead guilty to having been caught for a while by one of the immortal works of Lagrange. I may have told you that Lord Brougham—gilding the pill with praise more high than I could have expected—was pleased to allude, unmistakably, in an old *Edinburgh* [*Review*], under the head *False Taste*, to an expression of mine* which had spoken of Lagrange's great work, the *Mécanique Analytique*, as a "sort of scientific poem." To be sure, his lordship committed, on that occasion, the *slight* mistake of confounding the said work with Laplace's *Mécanique Céleste!*—at which blunder your father would smile. But Lord Brougham is a fine fellow for all that; and I think that De Morgan told me he had *seen* him reading my *Quaternions* in some railway carriage, or elsewhere. He has been lately writing to the French Institute on some supposed difficulties in the Integral Calculus.'

Towards the close of this year, prompted by the receipt from Professor De Morgan of a pamphlet on *Decimal Coinage*, Hamilton entered with zeal into a discussion of the question, showing himself to be a decided advocate of the proposed change. In the following spring he was officially applied to by Lord Monteagle, then Chancellor of the Exchequer, for a statement of his opinion on the subject, and he bestowed upon it a large amount of careful thought. As there can be little doubt that a change in this direction must eventually be effected, in the interest of international intercourse, of commercial account-keeping, and of economy of time, and money, and brains in national education, I think it right that the name of Hamilton should be allowed its weight on

* In the first Essay on Dynamics, *Philosophical Transactions*, 1834, part ii., p. 247; also in his Inaugural Address to the R.I.A. as President.

the reforming side, in conjunction with those of his great scientific contemporaries, Herschel, Airy, Babbage, Peacock, Whewell, and De Morgan; and with this view I insert a letter, written by him to an unknown correspondent, which sets forth his views; and I may refer those interested in the subject to the Parliamentary Blue Book, in which is inserted the draft of a bill drawn up by him to indicate how easily, as regarded legislation, the change might be brought about.* He learned, however, from his friend De Morgan, that a change so great affecting the coinage belonged to the prerogative of the Crown; and that the proper action of Parliament in the matter would be to address the Crown, praying it to exercise its prerogative.

The details of the proposed reform are fully discussed in the correspondence of Hamilton with De Morgan.

From Sir W. R. Hamilton *to* An Unknown Correspondent.

'Observatory, *October* 30, 1857.

'. . . As to the Coinage, I thank you for sending me the printed extracts, which I intend to enclose to my friend Professor De Morgan.

'The very clever writer is, as you are probably aware, quite on the opposite tack to my own: he is what he himself calls a "Little-endian," while I am decidedly, in the same mode of speaking (not by any means invented by myself), a "Big-endian." He wishes to go up from the *Penny;* I wish to go down from the *Pound.* This question must, of course, remain an open one until an Act of Parliament shall close it. But, in the meantime, I must protest against the notion that 6000 tons of our present copper money, or even so much as a single ounce thereof, must of necessity be withdrawn from circulation if the Pound and Mil system shall be adopted. It will be only necessary to declare that henceforward the *coin*, a "Penny," shall be legally the "thousandth part of a Pound." *Not a single new coin, nor any one new word,*

* *Sessional Papers* of 1857, Sess. 2, vol. xix., *Questions communicated by Lord Overstone, with Answers*, pp. 181, 182.

will *need* to be introduced; though cents and mils will almost certainly come into play. A new *relation* of value of *copper* coins to our *silver* and *gold* ones will be the whole and the slight alteration which will be absolutely required. This bugbear of a requisite recasting of our copper coinage amuses me; but what would it be, if it were real, to the enormous and really intolerable inconvenience of parting with our shillings and our pounds, or of having francs circulating, with only an approximate resemblance to the French francs in value, and with too near a likeness to our present shillings in their form?

'I need not, of course, dear sir, remind you that our present word, "Penny," which I by no means *wish* to get rid of, is used now by us, in these countries, with a *very different* signification from the Scriptural one; and, therefore, why may we not *alter its meaning* a *little* more? At all events, I hope that all the foregoing remarks may be held by you to have been within the fair limits of discussion, and that you will believe me to remain,' &c.

A Paper by Hamilton, dated Sept. 29, 1857, appeared in the *Phil. Mag.* for the following November. It was entitled: *On the Calculation of the Numerical Values of a certain class of Multiple and Definite Integrals.* The object of the Paper was 'to illustrate some points in the theory of functions of large numbers, and in that of definite and multiple integrals.' In it he extends investigations of Poisson, and deals with integers, running to as many as sixteen digits, with seven decimals attached; and speaks of having found the sum of the first *sixty* terms of one integral developed by means of an ascending series. Airy, acknowledging the receipt of a copy, thanks the author for 'your Paper on tremendous Integrals.' Hamilton, however, was not content with the extent of treatment given to the subject in this Paper. In the succeeding year he carries on its discussion in many letters, two of them of portentous length, addressed to Professor De Morgan. The first of these two letters (commenced Feb. 15th, 1858, and finished May 22nd), of which De Morgan writes, that it deserves to be entitled a collection of memoirs, extends to seventy-two closely-written folio pages; the second (dated July 15th, 1858) to twenty-four similar

pages. In the latter he discusses, in comparison with his own methods, that contained in a Paper contributed by Professor G. G. Stokes, Lucasian Professor of Mathematics, and now President of the Royal Society, to the *Transactions* of the Cambridge Philosophical Society (vol. ix., part 1).

On the 14th of December in this year Hamilton wrote to Professor De Morgan a letter of considerable scientific interest: its object was, in his own words, to set forth 'a natural mode of transition from Argand's method to my own, or from Double to Quadruple Algebra'—'how that passage *might have been made.*'

CHAPTER XLIII.

FESTIVAL OF POETS. VISIT TO EDGEWORTHSTOWN. DONATI'S COMET. PROFESSOR TAIT. BURNS' CENTENARY.

(1858.)

It is to be remembered that, whatever might be the scientific investigations to which Hamilton occasionally turned aside, his main occupation during the years now arrived at was the preparation of his Manual of Quaternions, to which he subsequently gave the name of *Elements of Quaternions*. Becoming aware of the imperfection of his *Lectures* as a treatise, he determined to make his second book complete and satisfactory, and to this end he very carefully laid down the lines upon which the structure was to be raised. He did not anticipate, however, the magnitude which that structure would assume, nor the time which its building up would occupy. He thought that a volume of 400 pages would suffice for a work which 700 did not bring to its completion, and he hoped to publish within two years what occupied him to the day of his death, more than seven years after the commencement of the undertaking.

It was from this great task that he allowed himself occasionally such diversions as the Paper on Definite Integrals recorded in the last chapter, and the long letters on the same subject which employed a portion of his time in the earlier part of 1858. But 'non semper arcum tendit Apollo.' The reader will be glad to catch a glimpse of the laborious mathematician taking a real holiday on one fine day of this Spring (the 23rd of April), when a few congenial friends visited him at the Observatory. The day was frequently referred to afterwards by himself and those who

enjoyed his hospitality as a festival most happily devoted to Friendship and the Muse. For the meeting was one of Poets. To Hamilton were added Aubrey De Vere, his dearly loved companion-friend; Mrs. Wilde, the erewhile enthusiastic "Speranza" of the *Nation*; John Anster, the early translator of *Faust*, a man never to be forgotten by those who could appreciate his fine taste, his wide knowledge, and the summer-lightning of his flashing, playful wit; and Denis Florence MacCarthy, an accomplished original poet, and known also as a graceful translator of some of the plays of Calderon. The well-beloved garden and the nearly-equally loved sloping field below the little iron gate, were the scenes of merry and serious converse, of recitations and readings, including both original effusions and poems by great masters of the lyre, recalled to memory in the course of critical discussion. A letter of Aubrey De Vere's may be accepted as a sufficient reminiscence of the réunion. I insert it in preference to a longer and fervid outpouring of MacCarthy's on account of its pictorial description of Torquay, the place from which it was written.

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'ENMORE, TORQUAY, DEVONSHIRE, *May* 14, 1858.

'. . . I hope you have been prosperous since I saw you. I shall not soon forget the pleasant day we had with you: our merry dinner, rambles about the green fields, and poetical recitations. I thought we should have met again afterwards, but you never made your appearance in Dublin; nor was I more fortunate as regards your friend "Speranza," at whose house I called in vain. I sent her also the day after our party my *Search after Proserpine*, and Mr. De Vere's *Misrule and Misdeeds* (as some of my *English* friends call my vindication of Ireland), but I do not know whether they ever reached her. So, when you meet her next, pray tell her that the books were sent—the poetical one to abate her appetite for the *Waldenses*, and the *rebellious* one to revive certain sentiments of patriotism which

were once hers, and which would never have been severed from *Hope*, had they not been connected too much with certain Repeal illusions, and too little with Ireland's true cause—what that is I will not say.

'Have you ever been on this southern coast of England? It is very lovely—bright, beaming, and genial. The villas are so thickly scattered, that if the blue of the water were deeper, and the green of the fields and trees more harmonised with brown, yellow, and that penitential and ashy-grey of the olive, with which Italy atones for the luxuriance of her orange groves, one might almost imagine oneself on the shores of Salerno. The Ministry seems on its last legs, and the Whigs likely to have their turn again. I suppose the oftener the poets and the critics of the political world—the Governments and the Oppositions—have to change places, the better for the sobriety of both parties. Adieu.'

A reference to that day of pleasure made by Hamilton himself in a letter to Mrs. Wilde will interest the reader: it tells eloquently how fitted he was to enjoy one of the chief delights of spring, and brings out into full view the conflict of feelings which his intercourse with his beloved friend Aubrey De Vere then stirred up.

'*May* 13*th*, 1858. . . . I am unable to recall—so much of human music was there in the poetical party at which you were so kind as lately to assist—whether the *birds* were singing at that time. This morning I have unlocked the hall-door, that I might listen more freely to the storm, the tempest, the whirlwind of delight, and of music, with which the birds are now surrounding this house and me.

'May I pass abruptly to a quite different subject, and express a hope that Aubrey De Vere will not succeed in converting, or in perverting you? . . . He talked with or to me, for about two hours, during our walk to and near Abbotstown, a little before your last visit, continuously; no beauty of Nature seemed able to win him, for even a moment, from his intense contemplation of what he regards as the "Glories of Mary"; and I confess

that I parted from him with a feeling of fatigue*, though also with a sense of admiration, for a high and unselfish—which yet I must judge to be a mistaken—aim. But I do not forget the conversation, to me interesting, which I enjoyed with you about a year ago, and in which I expressed an admiration of Aubrey, as being a "Knight of the Virgin." You know, perhaps, the *Lago Lugano* of Henry Taylor, but will suffer me to copy for you one stanza here:—

VII.

"Our book for us: of amaranthine hues
The flowers that to the free but searching sight
Did then disclose their inmost beauty bright!
Flowers were they that were planted by the Muse
In a deep soil which the continual dews
Of blessing had enriched: no lesser light
Than what was lit in Sidney's spirit clear,
Or given to saintly Herbert's to diffuse,
Now lives in thine, De Vere."

The lines are not very fine, and on the whole the author of *Van Artevelde* may be said to excel rather in good sense than in poetry. But I accept fully his praise of my friend. In one of my conversations, at Castle Ashby, with Lady Marian Compton, since Alford (who designed that beautiful engraving of the Virgin and Passion Flower which you have seen), I said that in all my many talks with Aubrey there had been nothing said which a lady might not hear.'

A few brief excerpts from notes to Aubrey De Vere tell of meetings with men by whose conversation he was interested:—

'*May* 28, 1858.—Professor Hennessy, along with Mr. Graham (the discoverer of the Irish Planet, Metis,) breakfasted here yesterday, and we enjoyed much intellectual talk, which had scarcely any relation to science, technically so called. Hennessy

*Will you quote against me the lines of Milton (Book VIII.)—

"He ended, or I heard no more, for now
My earthly by his heavenly overpower'd,
Which it had long stood under, strain'd to the height,
In that celestial colloquy sublime, . . . "?

has since sent me a copy of the *Dies iræ, dies illa*, of which I had only seen a part, namely, the extract in Goëthe's *Faust*.'

Hamilton expresses his admiration of this grand old Hymn, but his characterisation of it does not seem to me to be sufficiently striking or appropriate to call for reproduction.

'*June* 10. . . . I have been very busy since we met, though I have also had a dinner-party here—more formal than the one you last assisted at. It was, however, as I hope my guests thought, a pleasant party. Waller and Anster were two of the company, and there were some young men too.'

A reminder to Anster of the invitation to this party brings out some points of character and circumstance not without interest.

'*June* 3.—Dr. Waller *will* come; so perhaps you can come together; but if anything induces or enables you to come an hour or two earlier, do so. You will find a garden and books, among which, I regret to say it, thanks to the partiality of a learned lady of several years ago, you will *not* find a copy of the translation of *Faust* by a certain DOCTOR of my acquaintance. Do you know that I think that one of the *vainest* points about me—though *that* may be saying a great deal—is the pleasure which I feel in being, as a "Doctor," a fellow of yours and of Waller's? I have knelt before *two* Vice-Chancellors, to receive a Doctor's degree, and have (must I confess it?) strutted as a "Don" in Cambridge, insignificant as I may be in Dublin society: except that you and Mrs. Anster, and a few others, admit me to a certain cordiality.' *To* AUBREY DE VERE. '*June* 12.—Something interrupted me yesterday, and I have been occupied for several hours to-day in receiving a large party of students from the College, who came (by appointment) to see the instruments, and who have since taken an early dinner with me.' '*June* 14.—Another line of mere acknowledgment, to say that I have been integrating differential equations at a great rate lately, and that when I hoped to answer some of your notes this morning I was called on by a very agreeable young American gentleman, Mr.

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Irving Grenell of New York, a nephew of Washington Irving (about whom he told me some interesting things), and a connexion of some other celebrated people. Another uncle of his fitted out chiefly, if not solely, at his own expense, an expedition in search of Sir John Franklin. I can't write another line at this moment, as Mr. Grenell's visit has occupied most of my morning: we walked together through the fields to Dunsinea, for Miss Rathborne had invited us all to lunch there; but (being busy) I left him at her hall-door, though some of my inmates remained there to luncheon, and though I had forgotten to eat *anything* to-day.'

Hamilton, it will have been remarked, felt himself insignificant in Dublin society; yet, besides the exceptions to which he refers, there were eminent members of that society, in various degrees of intercourse with him, by whom he was justly appreciated. Of these was the then Lord Lieutenant, the accomplished Earl of Carlisle, who with the following words concludes a note, in which he gracefully thanks Hamilton for some presentation copies of his recent mathematical and poetical productions:—'*March* 8, . . . And now let me me heartily thank you for all the consideration and kindness I have met with from you.'

At the end of July, by the invitation of Mrs. Edgeworth, Sir William and his eldest son, William Edwin, joined a large party at Edgeworthstown to celebrate a festival in honour of William Edgeworth, whose father, no longer among the living, had been Hamilton's old friend, Francis Beaufort Edgeworth, and who himself had been schoolfellow of the son who now was Hamilton's companion. On the return of Lieutenant Edgeworth with his regiment from India, the tenants of Edgeworthstown had combined to testify their joy by the presentation of a sword of honour, and were, in reciprocal kindness, invited to a banquet at which congratulatory speeches were made in good old Irish fashion. Hamilton enjoyed greatly this opportunity of meeting the members of a family with whom he had been long bound in friendship and intellectual sympathy. Besides the respected lady at the head of the family, who was his hostess, he met Dean and

Mrs. Butler, Mrs. F. B. Edgeworth, the widow of his friend and mother of the hero of the day and of other children. To one of these, who now ranked as Miss Edgeworth, he addressed the sonnet which I here present to the reader as the latest extant of his verse compositions. A letter to his daughter, written a day or two after the festival, tells of his having visited the church. 'On Friday I went with Richard to the church tower, and climbed (not without danger) to the top of the tower, whereon the spire rests, having been erected on a peculiar plan by the late Richard Lovell Edgeworth,* father of Miss Edgeworth, the Authoress.'

The interior of the church contained memorial tablets of the Edgeworth family, and among these one met his eyes recording in brief simplicity the birth and death of Maria Edgeworth. Under the impression of this sight, his thoughts toned down to a correspondingly stern simplicity, he wrote the following lines:—

To MARY EDGEWORTH.

Mary! your great MARIA ne'er from me
Took tribute of such song as I could give:
Though deep entreasured in my bosom live
Thoughts of her frank and loving courtesy,
Her wisdom, wit, and truth. 'Twas sweet to see,
Yet sad, to-day, memorial tablets brief,
Less speaking of the fond survivors' grief,
Than uttering, in that simplicity,
Which with her noble nature suited well,
Such record grave, and not of her alone,
As fitly might on monumental stone,
Within those holy walls to which the bell
From tower ancestral calls with solemn tone,
Her birth, her death, her name of EDGEWORTH tell.

'EDGEWORTHSTOWN, *July 23rd*, 1858.

The autumn of this year was saddened to him by the dangerous illness of his daughter, then staying with her cousins at Trim. Her illness was of so alarming a character as to oblige him to give up the intention of attending the meeting of the British Associa-

* See *Nicholson's Journal*, vol. xxx. p. 241, December, 1811.

tion at Leeds, and to summon down to Trim, for repeated consultation, his friend Dr. Stokes, by whose wise treatment the life so precious to her father was, after long struggle and a tedious convalescence, restored to comparative health. Upon his return to the Observatory, in September, the great comet of Donati occupied Hamilton's attention as Astronomer, and he incidentally makes mention of his being engaged through two entire nights successively in observation of the splendid apparition. Much was he beset by inquiries on this subject from amateur astronomers, but one of these correspondents, the Hon. Mrs. Ward, to whom he had been introduced in the previous June by his friend Mrs. Smythe, of Farmleigh, proved herself so well-informed, and so much in earnest as a student of the science, that he could not but cordially assist her design of adding to her record of recent comets some history of one which surpassed them all in lustre and magnificence, and he obtained for her the grant from the Royal Astronomical Society of its Monthly Notices. This accomplished lady, who was a cousin of Lord Rosse, and by marriage connected with the family of Lord Bangor, published, in 1859, a book entitled, *Telescope Teachings*, illustrated by her own drawings, and giving much information respecting Donati's Comet. She had previously published a similar work, entitled *The World of Wonders*, of which Hamilton writes, 'it might as well have been called *Microscope Teachings*'; and also a work on *Entomology*. Her life was brought to an untimely close by a carriage accident in the neighbourhood of Birr Castle, where, as she reports in one of her notes to Hamilton, on the gallery of the Great Telescope she 'had more than once stood in bitter frost long after midnight.'

But the month of August brought to Hamilton an introduction of a scientific correspondent, who met him on the ground of his own special investigations, and was qualified to be in them not only a learner but a companion-explorer. A letter from Dr. Andrews, Vice-President of the Queen's College, Belfast, sought from Hamilton the favour of allowing PETER GUTHRIE TAIT, then Professor of Mathematics in that College (now the well-

known Professor of Natural Philosophy in the University of Edinburgh), to correspond with him upon the subject of Quaternions. The favour was granted, and it may be said that if it was a great advantage to Professor Tait to be instructed and guided by its discoverer in this new region of science, Hamilton, also, while carrying on his great solitary labour, had reason to feel grateful to his able and zealous disciple for valuable sympathy and stimulus. The correspondence which began in this autumn was continued up to the last year of Hamilton's life with a persistency and vigour which must have appeared wonderful to the younger man. One incident of it must be recorded.

Professor Tait had in view to publish, with Hamilton's sanction, a set of examples in Quaternions which might afford popular proof of the utility of the calculus; Hamilton, on the other hand, desired that its publication might be deferred until his own work should appear. So wide was the range, so ample was the detail of this work, that its progress was disappointingly slow, and Professor Tait's natural eagerness to bring out his own book, of which a public announcement had been made, and to turn to other researches, did at one time cause Hamilton uneasiness, lest the interest of his *Elements of Quaternions* might be thus forestalled. But this uneasiness was happily dissipated. Hamilton refrained from all expression of complaint or remonstrance, and was thus enabled to welcome, as it deserved, the honourable spontaneous action of Tait, who not only took an early opportunity of publicly acknowledging, in amplest terms of homage, the supremacy of the discoverer in the territory of Quaternions, but gave frank engagement not to anticipate Hamilton's publication. The cloud passed from Hamilton's mind: he was pleased and satisfied with the loyalty of his disciple, and in one of the latest notes of his *Elements** he designates him as 'eminently fitted to carry on, happily and usefully, this new branch of mathematical science, and likely to become in it . . . one of the chief successors of its inventor.'

* *Elements of Quaternions*, p. 755.

A very early letter from Professor Tait contained a paragraph which gave deep intellectual gratification to Hamilton, both as a testimony to the merits of his Calculus, and as an indication of the competency of the learner who was beginning to wield the instrument.

From PROFESSOR TAIT *to* SIR W. R. HAMILTON.

'QUEEN'S COLLEGE, BELFAST, *January* 3, 1859.

'. . . About Quaternions in general, I may remark (as indeed I very frequently feel), that the processes are sometimes *perplexingly easy*—by which I mean that one is often led in a step or two, and without at once knowing it, to the solution of what would be, by ordinary methods, a work not so much of difficulty as of labour. This, however, I take it, must form one of its great excellences in the hands of a person very well acquainted with it. A drawback to a beginner, but (as I am gradually being led to perceive) an immense advantage to one well skilled in the analysis, is the enormous variety of transformations of which even the simplest formulæ are susceptible: a variety fully justifying a remark of yours (*Lectures*, Art. 504) which, not many months ago, used sometimes to puzzle me. If I had gained nothing more by reading this subject than the facility of making problems and transformations for examination papers (especially in Trigonometry), and so saving an immense amount of time and trouble, I should have considered myself amply rewarded; but I hope in time to be able to apply it to perfectly original work (if anything *can* be quite original in these days). I make these remarks because you expressed yourself willing to hear anything I had to say on the subject, and because at present they are indissolubly connected with all my ideas on Quaternions . . .'

This letter was seen by Dr. Romney Robinson, who thus comments upon it:—

From T. ROMNEY ROBINSON, D.D., *to* SIR W. R. HAMILTON.

'OBSERVATORY, *April* 18, 1859.

'I know Tait, and think highly of him. It is very satisfactory to see one of such energy taking up the Quaternions with a firm

grasp, and from his position he is likely to spread a knowledge of their value and power. The surprise which he expresses at the startling simplicity of some of the processes must, I think, be felt by everyone, and is perhaps that which first opens the mind to a full conviction of the extraordinary value of this branch of analytic science.'

It is only due to Professor Tait that I should not withhold another passage from a letter written by Hamilton in December, 1858, on account of the testimony it bears to the progress made by Tait in the early stage of his study of Quaternions.

'. . . I have read . . . the first sheet of your "Quaternion Proofs," and must say that they appear to me to be *wonderfully elegant*, and to exhibit a very remarkable degree of *mastery* (so far) over the Calculus of Quaternions, used as an *instrument of expression, and of investigation*. It would interest me much to know whether (previously to our present correspondence) you had received *any* assistance from any other student of that Calculus. Or did you learn *all* that you had acquired from the *Book* itself, combined no doubt with your own private exercises, of various sorts? If the *Lectures on Quaternions* have been your *only* teacher, I must consider the result of such a state of things to be not only creditable to *your own* talents and diligence, but also complimentary to, and evidence of, some *didactic capabilities* of my volume; which ought to tend to console me under my artistic consciousness (as an author) of so many faults of execution, that if I could afford the expense of bringing out a *new edition*, I should be more likely to make it a *new work*. In Dublin, indeed, there exists a little "School" of Quaternionists, developed partly by the Lectures and Examinations which C. Graves and myself have given and held; and the Professor's brother, my old friend, John T. Graves (repeatedly mentioned in my Preface), called my attention about a year ago to a highly favourable and very elegant article in the *North-American Review*, for July, 1857, on the subject of the Quaternions and of my book. But a conscientious author wishes rather to be *read* than to be *praised;* and therefore I should like to be informed what drew your attention to my book, and whether you had any personal assistance in studying it?'

In reply, Professor Tait gives an interesting history of his study of the subject from the beginning, and concludes—

'So you see that if there is any credit in my progress, it is entirely to your lectures and letters that it is due.'

An early letter in this correspondence contains a short sentence, which I must here introduce on account of the light it casts on Hamilton's early scientific ambition. In it M. Chasles, the Historian of Mathematics, is called to account for attributing to Descartes the discovery of a general property of cones of the second order, which had been stated more than 2000 years before by the 'Great Geometer,' Apollonius Pergæus. Hamilton's mention of the name of Descartes prompts the parenthetic addition—'who has been, however, to me an object of admiration—might I dare to say of rivalry—from my boyhood.'

In the last month of 1858 Hamilton, in a letter to John Nichol, speaks of being 'deeply gratified by a recent compliment from Glasgow.' What this was appears from a passage in a letter of the succeeding month to Professor Tait, in which he says, 'I had intended to talk a little about the approaching Centenary Festival in honour of Burns, to be held at Glasgow on the 25th of the present month, to which I have received a special invitation. I consider that invitation to attend "as a guest" upon the occasion a highly gratifying compliment, and I wish very much that I could avail myself of it, but fear I shall not be able to do so. . . . I am partly Scotch by my father's side, and always enjoy very much my little run to Scotland.' It would appear that, finding how deeply Hamilton's feelings had been moved and gratified by the public invitation to attend the Festival, his friends Dr. and Mrs. Nichol urged him to make the necessary exertion, and to give them for the second time the pleasure of having him for a few days the inmate of their Observatory. He found, however, that he could not with warrant of conscience yield to the attraction of his friends or indulge his admiration for the Scottish Bard. I give a letter to John Nichol, written immediately before the day of

commemoration. It is interesting, as expressing his general estimate of Robert Burns, and as revealing links of thought which brought the poet specially close to his affection and sympathies. Wordsworth's genial and indignant letter in defence of Burns, a gift to Hamilton from the author,* had, as the markings in the margin show, been read by him with cordial concurrence, and he has transcribed at the end of it the fine sonnet of Keats, which concludes with the apostrophe:—

> 'How glorious this affection for the cause
> Of steadfast genius, toiling gallantly!
> What when a stout unbending champion awes
> Envy and Malice to their native sty?
> Unnumbered souls breathe out a still applause,
> Proud to behold him in his country's eye.'

From SIR W. R. HAMILTON *to* JOHN NICHOL.

'OBSERVATORY, *January* 22, 1859.

'I cannot tell you with what reluctance I bring myself, at the last moment, to decline the invitation of your father, and of Mrs. Nichol, to be their guest next week during the celebration of the Centenary of Burns.

> "'Twere worth ten years of peaceful life,
> One glance at their array."

There's a very sincere quotation for you; but you will probably disdain to use it in any speech of yours, as you (most justly) rank Burns higher than Scott: I need not say than our Moore, whom I may have praised too much (I admit it) at Charlemont House a few years ago. But one does not count one's words of praise, in speaking of a friend, or even of an acquaintance, recently dead; and I remember that one of my half-sentences (which were well received in *Dublin*) was that "to know Moore, even a little, was to love him."

'Burns was a grand *Man*. I am not going to *praise* him; I

* Vol. i., p. 349.

leave *that* to Scotland and to *you;* supported, and sympathised with, by the universal heart of humanity. All this is so very well known that it has almost degenerated into common-place. Miss Edgeworth once remarked to me that such or such a thing "had been said till it was *not* believed"! A splendid remark, as I thought at the time: but the fame of your Burns can survive it.*

'I will just tell you one thing, which has pressed itself more than once upon my mind in relation to your great National Poet: What could have been done for him? *My* solution of the problem would have been, that he should have been allowed *more liberty,* and have been less interfered with, on the part of his superiors in station. In my own case, at all events, I am satisfied that the generous (for such to my *feelings* it has been) *non-interference* of my academical and other superiors, has both allowed and encouraged me to do *much more* for the public than I was likely otherwise to have done. I have, perhaps, *purchased* Freedom by sacrifices; but, at least, I *possess* it. As to making him a Prime Minister of England, I am of Mr. Burchell's opinion, and in a corner cry out, "Fudge!" Now mind that you will not, the least little *bit* in the world, *offend* me, if you shall take up this remark, abstaining, of course, as you would be sure to do, from mentioning me, and tear it all to pieces. How I should like to see its fragments fluttering in the midst of some eloquent oration of yours! though I might retain my own opinion after all. Affectionately yours.'

* 'Do you know that I am very strongly reminded of my own father, Archibald Hamilton, Esq., of Great (or Old) Dominick-street, Dublin, who died while I was very young, and left me only an armorial seal, and a few books, chiefly Oriental—for he had thoughts of getting me into the East India Service, and had some interest in that direction—when I look at a clever engraving in my possession, of BURNS, receiving the "Wear thou this," from Scotia's genius?'

CHAPTER XLIV.

CALVINISM. DATING OF SCIENTIFIC WORK. MANSEL'S BAMPTON LECTURES. BRITISH ASSOCIATION AT ABERDEEN. PORTRAIT. HERSCHEL ON QUATERNIONS.

(1859.)

AN important expression of opinion, on a great religious and theological question, is conveyed by Hamilton in a letter written early in 1859 to Dr. Mortimer O'Sullivan, a clergyman noted in his day for his eloquence in the pulpit, and for ability as a controversialist, but less credited than he deserved to be for largeness of view and freedom from personal animosities. This letter of Hamilton's is a protest against Calvinism, both as in itself a misrepresentation of Christian theology, and as leading, through mistaken identification of it with Christianity, to the discredit and even rejection of the Christian faith. Although the extract I here produce is followed in the original by a request that the letter should be considered private, I have thought that I should not now be warranted in suppressing Hamilton's opinion on so profoundly interesting a question—an opinion delivered with earnestness and deliberation by a man who was at once a reverent student of Holy Scripture, and a deeply-founded philosopher.

From SIR W. R. HAMILTON *to* REV. MORTIMER O'SULLIVAN, D.D.

'OBSERVATORY, NEAR DUBLIN, *February* 25, 1859.

'In the midst of mathematical and other engagements, I feel an irresistible impulse to *thank* you for the publication (not the gift) of a couple of anti-Calvinistic sermons which have fallen into my hands within the last day or two.

'To me it appears to be of *vital* importance, that a protest against Calvinism should be raised by people like you—if there be many such. And this I say, having a very dear sister living, whom I very much respect as well as love, and who is a devoted Calvinist; being also—*notwithstanding*, as I might almost be tempted to say—but really I ought not to say it—an extremely pious and practical Christian.

'With her deceased friend, Mr. Krause, I had, through her, some very slight acquaintance, which I valued. The beauty of holiness was seen in the man, and appears also in his sermons, of all of which I have purchased copies. But even Mr. Krause has completely failed to turn me into a Calvinist.

'I repeat that I regard this question as a *vital* one—as of course the Calvinists themselves would say. If the alternative were proposed to me, Whether I would be a Calvinist or an Infidel?—I dare not finish the sentence.

'But that unwritten conclusion has been, is, and will be thought, spoken, written, and acted out, by other men. I have studied, to some extent, the infidelity of Burns, of Byron, and of Shelley; and am satisfied that it was not Christianity, but Calvinism, which revolted them. May God have mercy on their souls! and on those who misrepresented Christianity.

'Of course I have read remarks on the "happy inconsistency" of human nature, and all that. Very consoling, to be sure. But still I shall be happy to be informed that *you* proceed to denounce, and batter, with a sledge-hammer power of reasoning, and also with all that wit, grace, fancy, and (if I dared to say it), humour, which you *cannot help* admitting into your publications, a System of Theology, which, if it had been imposed by external authority upon me, would have gone near to making *me* an infidel.' . . .

The value set by Hamilton on truth and accuracy, prized by him in all matters, but here inculcated in reference to the veracity of scientific publications, is proved by the following letter to the present Provost of Trinity College, who at the time was one of the Honorary Secretaries of the Royal Irish Academy. Hamilton's insistence upon the obligation of exactness as to dates, &c., in publications affecting, it might be, questions of priority in dis-

covery, could not have been addressed to anyone more entirely in sympathy with him as a lover of truth and justice than Mr. Jellett.

From SIR W. R. HAMILTON *to* REV. J. H. JELLETT, M.A., F.T.C.D.

[FROM A COPY.]

'OBSERVATORY, *March* 27, 1859.

'I have received, by this morning's post, a small packet from Gill containing a proof and duplicate of my last communication to the R. I. A. I suppose that it is *you* whom I have to thank for this prompt attention to my wishes. In the cover, Mr. Dillon, for Mr. Gill, requests me to return the proof on *Monday*, that is to-morrow; for on this occasion at least I am compelled to borrow the Sunday for work.

'As you know that I have stood up for the rights of *authors*, or rather for one form of these rights—another of them consisting in a moderately *rapid* publication of their Papers—you will, perhaps, permit me to say that I also respect very highly, and would, if it were needful, endeavour to maintain the Prerogative (or more accurately) the *Duty* of an *Editor*.

'In the present case I admit that all requisite courtesy to the *Author* (meaning myself for the moment, as people usually *do* mean themselves, when they talk in an abstract style) is discharged—as in this last case—when *one clear day* is allowed (although, if it could be made a week-day on some future occasion, I should be better pleased) for the opportunity of that author's revision.

'And by *revision* I do not mean *re-writing*. Society allows *some* mystification; but the more absolutely pure, and sacred, *scientific* inter-communication can be kept from even *half*-truths, in statements, the better shall I be pleased; and (what is much more important) the better will the scientific world get on.

'Accordingly, I have made only the most minute, trifling, and purely verbal alterations—hardly five words being inserted or changed—in the proof which has this day reached me. I confess that I desire—though it is one of the slightest things that I wish for—to *take a date* when I communicate a Paper.

'Now, if authors are to be permitted to *add* and *alter*, beyond

some very narrow and fixed limits, I do not see how they *can* take any such date, in virtue of a printed record, so liable to be evaded. In my own case, I am resolved that there shall be no such evasion. Nor is this any new resolve of mine. If you will take the trouble of turning over those parts of the volumes of the *Transactions* of the R. I. A. which contain Papers of mine, you will see that I have on *several* occasions *taken pains* to state that I had availed myself of permitted opportunities to *recast* whole memoirs.

'I wish to annex a note—but under the head of *Note added during printing*—the insertion of which will cause (as I count) scarcely any trouble or delay. Tell me something about the Exiles, when you have a minute, and believe me,' &c.

A note of earlier date furnishes an explanation as to the 'Exiles': I give it because it shows that Hamilton, though not having been much in intercourse with Mr. Jellett, was attracted by his powers and character. It incidentally reveals also other facts not without interest.

From the SAME *to the* SAME.

'OBSERVATORY, *March* 17, 1859.

'Our little discussion at, or rather after, the Meeting of the Academy, which I must confess that I *enjoyed*, as I do everything which leads me to know more of *you*, will not (I hope) have disinclined you to take charge of the Two Pounds enclosed: one for the Goldsmith statue or monument, to which I have from the first designed to contribute; and the other for the Neapolitan Exiles, with whom I see your name associated as a receiver of subscriptions.

'P. S.—I often make it a condition that my name shall not be published, but on the present occasion it appears to me that a part of the value of the subscription will depend on the publication of the name.'

The Bampton Lectures of Dr. Mansel, which provoked against them the impassioned reclamation of Frederick Denison Maurice, were at this time the subject of discussion in Dublin as elsewhere,

and Hamilton was referred to for his opinion of them by his old friend, Dean Butler of Trim, writing on behalf of his neighbour the Rev. John C. Macdonnell, then incumbent of Laracor.*

The following letter addressed to Dr. Lee, Fellow of Trinity College, Dublin, and well known as a Theologian, exhibits a partial result of Hamilton's perusal of the book. It is to be wished that it met more fully the main contention of the author as to the nature of our knowledge of God, but what is said in it has a value that will be recognised :—

From SIR W. R. HAMILTON *to* REV. WM. LEE, D.D., F.T.C.D.

'OBSERVATORY, *July* 2, 1859.

'I am much obliged by your kind note received this morning on the subject of Mansel's "Bampton Lectures"; and, if it be possible, am still more gratified by your having been so good as to remember our little conversation respecting them at Jellett's party lately.

'I suppose that I told you that my valued friend, Dean Butler of Trim—my acquaintance with whom wants only a few months of being one of forty years—ordered not long ago his Dublin bookseller to send to me his copy of those Lectures for my perusal; with a request that I would give him my opinion upon them, especially as bearing on some points of philosophy, with which he was pleased to suppose that I might be familiar.

'Accordingly, I received the book, and have it by me still. *One* careful and continuous perusal I have already given to the Lectures themselves—besides some dipping into the Notes—with an ungrudging and unbounded admiration. Such is, I think, the proper *temper* with which to approach a work of genius and of learning, devoted to the highest interests of religion and humanity. But after *one* such admiring, and indeed reverential, *pupil-time*, of sitting at the feet of a new author, a student may be pardoned for presuming, or rather an honest teacher would (no doubt)

*Afterwards Dean of Cashel, now Prebendary of Peterborough: author of *Donnellan Lectures on the Doctrine of the Atonement.* London, 1858.

desire him, to take for awhile the converse attitude, of judge and critic. You remember, probably, that passage of the *Kritik*, in which Kant speaks of the necessity of Man's assuming that attitude with respect to Nature itself:—"An die Natur gehen, zwar um von ihr belehrt zu werden, aber nicht in der Qualität eines Schülers, der sich Alles vorsagen lässt was der Lehrer will, sondern eines bestalten Richters, der die Zeugen nöthigt auf die Fragen zu antworten die er ihnen vorlegt."

'Accordingly I am, just now, in a mood for *finding fault* with Mansel—trying, as it were, to pick (in thought) a quarrel with him: I hope not in *revenge* for his having extorted from me so large a share of admiration.

'The grand question which he raises in my mind, at this moment, is the following:—

'Does he insist too much on the limitation of the human faculties?

'For example, "Philosophy" is a grand and sacred word.

> "How charming is *divine* Philosophy!
> Not harsh, nor crabbed, as dull fools suppose,
> But musical as is Apollo's lute."

'I therefore do not presume to assert—nor to deny—that there may exist a "PHILOSOPHY *of the Infinite*." But I am sure that there does exist a "SCIENCE *of the Infinite*"; to wit, Mathematical Science. And with respect to Note 15 to Mansel's Lecture II.—to which Note Dean Butler, on behalf of a friend of his and mine, has recently invited my attention—I must just be permitted to repeat from it, though not with the author's application, four pregnant words:—"Callide, acute, nihil supra."

'This, however, is far from being my judgment, even in its *second* and (transitionally) *hostile stage*, respecting the lectures at large. I shall not be contented till I have read them several times over.

'Just now it seems to me that if there be anything which I shall have a difficulty in finally adopting, it will be the severe, and almost contemptuous, criticism of Dr. Mansel upon Kant's *Practical Reason* (*Kritik der Practischen Vernunft*): although perhaps more hinted than expressed.

'I have possibly been *bribed* to like that work of Kant, by the

circumstance of its having been *mentioned* to me in conversation—for our intercourse was *not always* monologue—by my illustrious friend, and (if I may dare to say so) Master, Samuel Taylor Coleridge; who *gave* me his German copy of the *Urtheilskraft*, through his own particular ally, Joseph Henry Green.

'May I confess that I have only as yet *heard* of your own work on the Inspiration of Holy Scripture; and will it be presuming too much, if I venture to suggest an exchange of publications?

'At all events, I am about to write your name in a copy of my *Lectures on Quaternions:* a very interesting and valuable commentary on a part of which work has been quite recently sent to me from Leipzig, by the writer of that commentary or article, namely, by Professor Moebius, author of the *Barycentric Calculus*, &c.

'And if you are so obliging as to send for me to the Royal Irish Academy a copy of your "Inspiration," I shall not only be personally gratified, but will undertake to *read* the book. I am,' &c.

Dr. Lee, in acknowledgment of this letter, states his view of the subject in words which may be read with interest. He says:—

'I have re-read Mansel during the summer with great pleasure. I am sure that the popular misrepresentations of his view arise from not noticing the broad sense in which he employs the term Theology, viz. the Science of the Divine Nature. Hegel and others regard this science as within the reach of human reason; and in opposition to this exaggerated estimate of man's faculties Mansel protests. I think I mentioned this to you already, and a re-perusal of the Lectures confirms me in this opinion. He may, of course, following your namesake's theory, err in his notion that the Infinite can in no sense be an object of Science; but this is a collateral question.'

At the end of July Hamilton escorted his daughter, still an invalid, to stay for some time with the Keating family, relations on her mother's side, at Rhyl, in North Wales. In the beginning of September he carried her off from these kind friends to become the guest of his own maternal relations, Mr. and Mrs. Joseph Willey, at Fulneck, the Moravian settlement near Leeds. Here

she was to remain while he attended the Meeting of the British Association at Aberdeen. It was presided over by the Prince Consort (whose opening address he speaks of having 'greatly enjoyed'), and brought together many scientific friends whom Hamilton was glad to meet again. Among the most eminent of these were Faraday, Lord Rosse, Airy, Brewster, Romney Robinson, Humphrey Lloyd, Murchison, Sabine, Henry Smith, the Abbé Moigno. But perhaps the personal contact most interesting to him on this occasion was his meeting in the flesh his epistolary friend, Professor Tait, by whom was introduced to him a man who, as we have seen,* became, after Hamilton's death, a successful employer in physical researches of the Calculus of Quaternions, the gifted James Clerk Maxwell. Hamilton's own contribution to the proceedings of the Association was a Paper *On an Application of Quaternions to the Geometry of Fresnel's Wave-Surface.* A special interest was attached in the mind of Hamilton to this communication, because, by an agreement between him and Professor Tait, both had been carrying on simultaneously, but in entire independence, an investigation by Quaternions of the same problems. Hamilton distributed a lithographed statement of his method of solution, besides making it the subject of an oral address to Section A, in preparation for which he pencilled in a manuscript book a note, which I here insert as showing what was his aim at this time in the development of his calculus:—

'Not about to lecture on Quaternions; nor do I pretend on the present occasion to produce any new property of that celebrated and important surface, which is mentioned in the title of this communication, but merely to *exemplify* the *conciseness* and *simplicity* with which the *language* of quaternions enables those who have acquired some familiarity with that language to *express* and *combine* general conclusions, and even *physical hypotheses*, and then to *transform* the resulting formulæ, and to *interpret* the new equations

* See vol. ii., p. 445.

so obtained. I said *hypotheses:* for I desire it to be distinctly understood that I do not by any means *adopt* any such hypotheses, nor at all pretend to *express*, or even to *form*, any *opinion* of my own on such a question as this: Are the *vibrations* of the ether (if such vibrations actually exist) *perpendicular* or *parallel* to the plane of *polarisation?*'

At Aberdeen Hamilton was the guest of Mr. James Westland, and, as on former occasions of a like kind, he appears to have gained the esteem of the whole family circle of his host. He enjoyed also, with a distinguished party, the hospitality of Mr. and Mrs. Alexander Thompson, of Banchory House, and was one of the two hundred members of the Association who, on the invitation of the Queen and the Prince Consort, visited Balmoral—a visit which untoward circumstances rendered less successful than had been hoped. Leaving Aberdeen for the south on the 23rd of September, he received the saddening intelligence of the death, on the 19th, of his much loved, accomplished friend, Dr. J. P. Nichol, of the Observatory of Glasgow. The friendship between them had been of quite a brotherly character, scientific knowledge and pursuits being in their case the acquisitions and employment of natures which derived their most prized enjoyments from elevated sentiment and poetic thought. This loss had the effect of binding Hamilton only the more closely to the son and daughter of his friend.

To rejoin his own daughter at Fulneck was now his aim, and at this Institution he again spent some days, the principal incidents of which were his giving a Lecture on Astronomy to the youthful pupils of both sexes (from whom letters of thanks were addressed to him*), and by his sitting, with his daughter standing at his side, for the photographic likeness from which an autotype reproduction has been placed as a frontispiece to the second volume of

* I have found a similar letter of thanks from the girls of Mercer's School, situated in his own parish of Castleknock.

this work. And here I take the opportunity of expressing my opinion that this representation of his features stands out from all other photographs of him which I have seen (and I believe I have seen almost all that were taken), as alone doing something like justice to the combined intellectual and moral character of the subject. It exhibits, I think, both in conformation and expression, the profound thinker, the reverent benevolent sage. The marble bust in the Library of Trinity College is from the hand of Foley, and a photograph from it supplies the frontispiece to the present volume. Our eminent sculptor never had the advantage of seeing Sir W. R. Hamilton, and had to work from small photographs and a cast of the anterior half of the head. The aspect which the photograph presents will, however, be acknowledged by all who knew the living man to be both fine and like.* Hamilton had now the happiness of recognising that the visits in England of his beloved child had been attended by the wished-for result, and in restored health she accompanied him on his return to the Observatory at Dunsink.

When again at home and at work upon his new book, he received from Sir John Herschel, to whom he had sent a copy of the Aberdeen lithograph of his quaternionic treatment of Fresnel's Wave, the following 'cry of distress.'

* In the course of this year Hamilton was much, and, as it appears to me, rather unreasonably disturbed by a suggestion made by General Larcom, that its ex-presidents should consent to their portraits being obtained for the meeting-room of the Royal Irish Academy. He felt that he 'could not afford' to present his own, and shrunk from saying so, and he strongly objected to what Romney Robinson, who sympathised with him, called 'the clumsy machinery of subscription' being resorted to, at least during his lifetime; this he considered would be 'humiliating' to him. He seems not to have considered that a good portrait could only be taken from life. The Academy and his country have, I think, great reason to regret a decision which conduced to the non-existence of a series of portraits which would have been of permanent historical interest. I refrain from printing the correspondence on the subject with Dr. Robinson and General Larcom, who was astonished at the ill reception which his proposal met with.

From SIR J. F. W. HERSCHEL *to* SIR W. R. HAMILTON.

'37, TAVISTOCK-PLACE, RUSSELL-SQUARE, *November* 18, 1859.

'MY DEAR SIR WILLIAM HAMILTON—Your deduction from Quaternions of Fresnel's Wave is one of those things which I have just knowledge enough to admire without enough to understand. But it set me again on reading your *Lectures on Quaternions*, and I got through the three first chapters of it with a much clearer perception of meaning than when I attacked it some three or four years back, but I was again obliged to give it up in despair. Now I pray you to listen to this cry of distress. I feel *certain* that if you pleased you *could* put the whole matter in as clear a light as would make the Calculus itself accessible as an instrument to readers even of less "penetrating power" than myself, who, having once mastered the *algorithm* and the *conventions* so as to work with it, would then be better prepared to go along with you in your metaphysical explanations.

'Do pray think of this. At the risk of offending, I will venture to say you will not have done yourself justice if you do not give the world some clue that a lower class of thinkers can unravel than those who alone can hope to master that book.

'The simplest way would be to give forth a number of *examples* of the treatment of problems and theorems by it. I mean not examples which shall be of themselves general theorems or important discoveries, but good honest ordinary problems or theorems, such as can be readily worked by common Algebra and Trigonometry, but gradually increasing in difficulty; and these might be prefaced by a clear statement of the Rules of the Calculus *as Rules*.

'Such a book would have an immense influence. Hundreds would learn to use the Calculus as a means of investigation and its theory would by degrees [be] popularised. Pray excuse this from yours very sincerely.'

Hamilton replied in grateful terms, and enclosed for Herschel's satisfaction the initial sheet of his proposed new work, written, as he trusted, 'in a *style* like that which you desire for me; or at least more like it than the *Lectures*.' He had the gratification of

receiving from Herschel the following acknowledgment of his specimen :—

From the SAME *to the* SAME.

'*November* 25, 1859.

'Nothing can possibly be clearer or more to the purpose, and if the rest of your book be as much so, and go on the same principle, it will be one of the most important, and I will venture to say, the most widely circulated *elementary work* ever published.'

A second sheet was sent by Hamilton to Herschel, and drew from him the criticism that by the introduction of some difficult applications of the Calculus the attention of the commencing student was too soon diverted from its principles. Hamilton submitted to the criticism, and re-wrote the sheet, thanking his critic by sending to Lady Herschel for communication to him the passage from Horace's *Epistola ad Pisones* (l. 438. . . .), in which the Roman teacher of his Poetic Art exhibits in Quintilius Varus the character and action of the critic who was a real friend. In a subsequent letter of Herschel, acknowledging further proofs, he first suggests the suppression of a passage respecting transversals, and then retracts the suggestion, and concludes with the words, '*Au reste*—the thing is CHARMING, and I can only add, Go on and prosper.'

CHAPTER XLV.

ANHARMONIC CO-ORDINATES. GEOMETRICAL NETS IN SPACE. GEOMETERS IN TRINITY COLLEGE. CIRCLE-SQUARING. AUBREY DE VERE'S 'INISFAIL.'

(1860.)

In the beginning of 1860 Hamilton, while carrying on his work upon the *Elements*, opens a mathematical correspondence with Dr. Hart, then Senior Fellow of Trinity College, now Sir Andrew Searle Hart, Vice-Provost. From problems connected with a circumscribed pyramid, and tetrahedra in general, he rapidly advances to what may be called a new calculus, independent of Quaternions, though advantageously employed in connexion with them, the calculus, or instrumental machinery, of Anharmonic Co-ordinates; and on the 27th of February he begins what he himself calls a 'prodigious' letter, which, becoming an extended treatise, reaches at last the 216th folio page of closely-written work, and is completed by a postcript of sixty-four similar pages, dated the following 28th of August. Some of the results arrived at were, during its progress, communicated to Dr. Salmon, touching, as they did, some of the problems published by him in his *Higher Plane Curves*, and he acknowledges their novelty and importance, while Dr. Hart in a letter, dated so early in the correspondence as April 6, says of them:—

'If your Anharmonic Co-ordinates should never be used again, they have already conferred a great benefit on geometry, by turning your attention to the large and much-neglected field of cubics, in which you have already filled many gaps, and placed in a new point of view a theory which we have been hitherto studying entirely through Salmon's glasses. If you succeed in compressing

so much matter into the modest limits of an "Appendix," it will be a wonderful achievement in the art of condensation.'*

In letters written to Aubrey De Vere on the 16th and 17th of March, Hamilton gives to his friend an interesting *aperçu* of what he had been doing on this subject.

'*March* 16.—Within the last three weeks I have written quite an essay—not to say a treatise—on (what seems to *me*) a *new* scientific system, or method, of what I call "Anharmonic Co-ordinates"; and which, wonderful to tell, appears new, *also*, to all the geometrical friends whom I have consulted in our University, *e. g.* Graves, Salmon, Ingram, Hart. It is strange that after a couple of millennia, a *thought*, which seems to be traceable to Euclid (through Pappus) should be found *now* to admit of a vast and unforeseen expansion. . . .'

Forwarding a copy of his sister's poem on Columbus, which his friend had asked for a month previously, he adds:—

'*March* 17. . . . One would think it was some heroic effort of virtue, my writing out a few lines of poetry for a friend: I put it off so long, and seem to make so much of work or fuss about it; although when I do begin, I experience really great pleasure in the act. If you could only see what quantities of less interesting (viz. mathematical) matter I write to my scientific correspondents!

* Such an Appendix is not to be found in connexion with the *Elements of Quaternions*, into the early part of which work are, however, introduced Sections dealing compendiously with both Anharmonic Co-ordinates and Geometrical Nets in Space. But from the fact that at the foot of page 34 reference is made to 'Note A on *Anharmonic Co-ordinates*,' and at the foot of pages 35 and 56 to 'Note B upon the *Barycentric Calculus*' ('*Nets in Space*'), which Notes are non-existent, it would appear that the author intended to annex an Appendix giving further details as to both these subjects. It must be remembered that the *Elements of Quaternions* was published after the death of the author, and is in an incomplete state. On the subject of Anharmonic Co-ordinates he made a communication to the Royal Irish Academy, which is printed in the *Proceedings* for the Session of 1859–60; and on the 24th of June, 1861, there was read by him, before the same Institution, a very important Paper, extending in print to above 50 pages, on *Geometrical Nets in Space*, a Paper carrying on to 'a Quinary Calculus for Space,' the researches which had in their earlier stage furnished him with his method of Anharmonic Co-ordinates.

—much faster, indeed, including the composition, than my assistant, or my son, can copy them for me. But I suppose that a man has instinct, as well as reason; the στοργή of the animal creation exists, and shows itself with me, when a new conception in science has dawned upon my intellect, and is in danger of altogether perishing, if not duly incubated in its season. My last conception, of a *geometrical* kind, has taken my friends in Dublin—who, on such subjects, are admitted to hold a very high place in the scientific world—entirely by *surprise* . . . But I expect to be quite cool again upon the subject in about a *month*. You once confessed to me that for a *day* or two after writing a new sonnet, you were not an impartial judge of it.'

I insert here extracts from the long letter to Dr. Hart, which have an interest derived from their recognition of more extensive knowledge than his own in modern geometry on the part of his mathematical colleagues in Trinity College. They are pleasing testimonies of his unjealous disposition, of his habitual desire to acknowledge points of superiority in others.

C. 1860, p. 38.—'*February* 27, 1860. I remember *staring* ten or twelve years ago when Townsend told me [in 1847], in what I call Brinkley's Garden, that a plane conic has double contact with each of its foci—considered, of course, as infinitesimal circles; and he admitted that i, j, k, which were then comparatively recent, had nothing in them more paradoxical, on their first appearance. You see that I have made a step or two forward since then, aided (no doubt) *mainly* by Salmon's books—though I know something of foreign ones also—in what is called the "Modern Geometry"; but I have the deepest feeling of my inferiority, in that respect, to persons who have made the subject their special study.' P. 151.—'*April* 25. As I cannot expect that you will do more than skim, at present, the sheets of this prodigious letter . . . I isolate thus a question or two in the hope of your answering them separately. You have been very useful to me already by calling my attention to parts of Salmon's book, which I had not sufficiently considered. His "Lessons," &c., I have got, and admire.' P. 241. —'The parallelism of $B''R$ to A, C, in figures 29 and 30 did not occur to me when I saw these figures, and 'tis perhaps curious that

I saw it half an hour ago by reasonings about *conics* many minutes before I was able to verify it by any special argument from circles. Such verification, however, should certainly have offered itself sooner to any of your Junior Freshman friends. (I was of course a Junior Freshman in my time, and was never beaten in Geometry—nor, as it chanced, in any other subject—in my division; but things have so much advanced generally in our University since *then*, that I might well meet with a different fate, in every department, if I could go in again *now*, as I have sometimes half-wished to do, to be lectured by Salmon, and Townsend, and others.) . . .'

This year brought to Hamilton the commencement of a correspondence, very unwillingly entered upon, with Mr. James Smith, of Seaforth, Liverpool, who about this time actively challenged notoriety as a squarer of the circle.* Hamilton felt obliged to repress his pretensions with greater severity of tone than was usual with him; I should scarcely have referred to the incident, had not the result been that in the course of the discussion he was led to communicate to the *Athenæum* (June 8, 1861), and afterwards in an extended form to the *Philosophical Magazine* (April, 1862) an 'Elementary Proof that Eight Perimeters of the Regular Inscribed Polygon of Twenty Sides exceed Twenty five Diameters.' Archimedes had proved more than 200 years B. C. that 71 perimeters of a regular polygon of 96 sides inscribed in a circle exceed 223 diameters, whence easily followed the theorem that eight circumferences of a circle exceed twenty-five diameters: a theorem contradictory to Mr. James Smith's assertion that they are exactly equal. Hamilton's 'Elementary Proof' served as an *à fortiori* argument to the same effect.

The death of Hamilton's youngest sister, Archianna, occurred in February of this year, when she was staying with her sister Sydney in Dublin. The event called into active exercise his affection and sympathy towards both his sisters. After an inter-

* See De Morgan's *Budget of Paradoxes*, p. 318. . . . , and *Athenæum*, *passim*.

val it was followed, as had been the death of his sister Eliza, by a visit to the county of Wicklow. In that well-known region's diversified beauties he sought as before, and experienced, the restorative influences of nature.

Extracts from Hamilton's correspondence with Aubrey De Vere principally on literary topics may fitly close the record of this year, and lead on to that of the next.

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'CURRAGH CHASE, *February* 21, 1860.

'... It would be well worth your while to look at a translation by D. F. MacCarthy of one of Calderon's "Autos Sacramentales" which appeared in the number for July, 1859, of the *Atlantis*. It is a very wonderful sort of poetry which has become better known in Germany than in England ... .'—*March* 15, 1860. 'I must just send you a line to tell you how sorry I was to hear of the death of your sister. I remember her perfectly. As life goes on, how many of those connected with one's earlier and brighter recollections have taken their place with those who have gone

"From sunshine to the sunless land."

'... Alas for the good old times, when we used to talk of Francis Edgeworth!'

From SIR W. R. HAMILTON *to* AUBREY DE VERE.

'*November* 30, 1860.

'I received your "Select Specimens of the English Poets," this morning, with very great pleasure, and have read much of it already, with the enjoyment for the larger part of recognition: but also with the new pleasure of reading your thoughtful, temperate, and wise remarks on the various authors, or rather (for the present purpose) poets, discussed. I also received about a fortnight ago a copy of your father's "Julian," and "The Duke of Mercia," which I value very much; and not the less, because I have certainly among my Curragh books, or memorials—in-

cluding presents from your father—a copy of at least "The Duke of Mercia." But "Julian the Apostate" had to me the air of a quite unknown, and of a wonderfully beautiful composition.'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'LONDON, *July* 19, 1861.

'I have requested M'Glashan and Gill to send you a new volume of poetry which I have just published. . . . The most important poem in it is one called "Inisfail," which occupies the latter half of the volume. When you have time to look into the book, pray begin with this poem, for I would like to have your opinion of it. It is a somewhat new idea in poetry, I believe. It is a Chronicle Poem, intended to represent in their essential features, both the history and the inner life of a nation and race, and to do so chiefly through songs and ballads, such as might have proceeded from the bards contemporary with the events described. There never was a more wonderful history than that of Ireland. Except a few picturesque and pathetic *details*, people commonly know nothing about it. They do not suspect that, seen from one point of view, and that the highest, so far from being a jumble of sad fragments, it has the highest possible Unity and Significance. Doubtless my point of view may not be that of my readers, in some cases. But poetry has a right to its *Postulates*, has it not? I cannot judge of the execution of course; but the theme is a good one, I think. *Nations* are the long-lived Patriarchs still allowed to survive, and work out their Destinies, *on earth*. They have a continuity not now given to the life of an individual; and their fortunes have thus an affinity with song.' *London, Nov.* 4. —'A few of the Reviews at this side of the water have been snarling at the Irish tone of my Chronicle Poem of "Inisfail," though I meddle with nothing within the last century. They cannot afford, apparently, even an epitaph to the Ireland of six centuries! What have you been about of late? Writing on mathematical subjects, I suppose, as usual. But you must find time too for an occasional sonnet also—"golden mile-stones" set up along the road of life. You were sorry, I am sure, for Elizabeth Barrett Browning.'

CHAPTER XLVI.

RECOGNITIONS FROM ABROAD. HONORARY DEGREE AT CAMBRIDGE. TAIT. INGLEBY. KIRKMAN.

(1861.)

THE beginning of 1861 brought Hamilton from abroad gratifying proofs of the extending study of Quaternions. Returning to Dr. Waller, Editor of the Imperial Dictionary of Biography, a proof, sent for critical correction, of a notice of his own life and works, he thus writes:—

'*April* 9, 1861. . . . I do not wish anything additional whatever inserted. . . . I think it may interest you to *know* that Dean Graves had lately received for me a copy of a very interesting and valuable work, in Italian, on the Quaternions, (Calcolo dei Quaternioni di W. R. Hamilton, &c.) by Professor Bellavitis; addressed on the outside, Al Chiarissimo Signore Prof. W. R. Hamilton, &c., which address my daughter (don't expose her) read out as if it had been Al Carissimo ("Fame is love disguised." Shelley). But this is only the last of several recognitions—and more than mere recognitions, since they attest successful *study*—of the Quaternions, which I have received from foreign countries, especially from Germany and America; for France is very slow to accept any truly *new idea*, although she *works* it well, when she has once caught hold of it. Something like a century had passed before Newton was appreciated in France; but the world moves now at a somewhat faster pace. I have long been, however, one of the six mathematical correspondents—technically so called—of the French Institute. (At least, I think the number is *six;* and am *sure* that it does not exceed *seven*, out of the whole *world* which to France is "barbarous," in the old Greek sense) . . . '

Recognition of Hamilton's work on a different subject from Quaternions, namely, *Systems of Rays*, arrived in February from Berlin. Dr. Meibauer presenting to the Royal Irish Academy his Paper entitled *De generalibus et infinite tenuibus luminis fascibus, præcipue in crystallis*, accompanies it with a letter in which, after claiming that he has extended some theorems of M. Kummer, he thus refers to Hamilton—honourably but amusingly designated—and to his prior results: 'But that my deduction for the special case (namely, the planes of vergency) may be correct, is clear thereout, that your great fellow, M. Hamilton, has already found, in a manner very different from mine, both principal theorems, that of M. Kummer and mine (*Transactions*, vol. xvii., p. 122, &c.).'

At the annual meeting of the Royal Irish Academy in March, he seconded, with a gratifying tribute of appreciation, the proposal by Lord Talbot de Malahide that Professor Charles Graves should for the ensuing five years be the President of the Academy. On his nomination by the new President to be the Senior Vice-President he wrote to his friend the following not uninteresting letter.

From Sir W. R. Hamilton *to the* Very Rev. Charles Graves, D.D., P.R.I.A.

'*March* 18, 1861.

'Although I shall, within an hour or two, have the satisfaction of seeing you in the Chair of the Council, yet I wish to state in writing that I accept with pleasure your nomination of me as the Senior Vice-President of the Academy, as I did the corresponding nomination of Lloyd on the occasion of his succeeding me. It was the first act of your prerogative and a graceful one—at least I have no reason to suppose that the Academy will judge of it otherwise. At the same time you will perhaps pardon me for saying again that the acceptance of the office, although one of which I am well prepared to appreciate the importance, was designed to be, on my part, a mark of the very great satisfaction, private and public, with which I saw you placed, on Saturday night last, in the Chair once occupied by myself, and both before and since by persons more deserving than I was.

'What I said in your presence to Dr. Reeves was, in essence though not in words, the admission of the natural feeling that having been for a while the first, it could be no object of *ambition* for me now to be the second in the Academy, yet I will say, on reflection, that it is a *pleasure* to me to be such for one year under you. For you will remember that when next year arrives, even if I should be then alive and well and on the Council, it will be *in due course* that my name should drop from your list of Vice-Presidents; and no member of the Academy, and least of all myself, could for a moment have imagined that any slight would have been intended, or conveyed, if you had lately used your prerogative otherwise than by appointing me. . . .'

An honour of a different kind summoned him in May to Cambridge. He received from the Vice-Chancellor, the Hon. and Rev. Latimer Neville, a notification that the Council of the Senate of the University desired to confer upon him, 'as a mark of their respect and esteem,' the honorary degree of Doctor of Civil Law. Having been in 1845, after the meeting of the British Association, admitted by the same University to the *ad eundem* degree of Doctor of Laws, Hamilton at first felt uncertain whether he was free to accept the proffered honour. He thus states his difficulty in a note to his friend Dr. Anster:—'I remember kneeling as a Candidate Doctor of Laws before the Vice-Chancellor of the time: is it lawful that I should kneel again for a degree no higher? Although perhaps in England it may be looked upon as such. *Anne aliquis bis baptizari potest?* For it was a *sort* of baptism—in the name of the Holy Trinity. For about sixteen years I have supposed myself to be a Doctor of Cambridge, and certainly wore there the Doctor's gown, where it counts for something in visiting public places,' &c.

Another consideration which weighed with him in his hesitation was his fear lest he might do anything to lessen the dignity of his Dublin degree.

He laid these facts before the Vice-Chancellor, adding that as part of the heading of a communication (on Symbolical Geometry)

to the *Cambridge and Dublin Mathematical Journal*, published in 1846, he had set himself forth as 'LL.D., Dublin and Cambridge,' &c. The reply was:—'The *ad eundem* Degree which you took is not the same *in point of fact* which we offer you, and therefore I hope you will be able to accept it.' The terms of his acceptance show how much he felt the honour. I extract them from his letter. '. . . Under your sanction I now accept the offer of the additional degree of D. C. L., which I shall very highly prize. Indeed I can scarcely imagine—or rather I am, at present, entirely unable to conceive—any more gratifying compliment which *could* from any quarter be paid to me. . . .' His visit to Cambridge brought him much pleasure. The ceremonial in which so honourable a part was assigned to him took place on the 21st of May. The distinguished men who shared with him the honour were Lord Stuart de Redcliffe, the Earl of Elgin, Dr. Gell (Bishop designate of Madras), Sir Roderick Murchison, General Sabine, Mr. Grote the historian, and Dr. Robinson of Armagh. He records that in the order of procession he came next after Lord Elgin, that he was in the speech of the public orator praised more than he chose to relate, and that before the banquet he was presented to the Prince of Wales, with whose manner—giving the impression of receiving rather than conferring honour—he was favourably struck. Murchison, Sabine, and Robinson were old friends; and other old friends he found in Cambridge in Mrs. Robinson, her daughter Mrs. Stokes, and Miss Mary Edgeworth, her niece, to whom were added Professors Challis and Adams. Dr. Whewell happened to be absent, and in a kind letter expressed to Hamilton his regret at missing him, and his pleasure that Lady Affleck had made his acquaintance. On leaving Cambridge, Hamilton took the opportunity of visiting what Whewell, a connoisseur in Gothic architecture, called in his letter the 'noble cathedral' of Peterborough.

He was again in his own Observatory on the 27th of May, and early in the following month had the pleasure of receiving there, for long converse during two days, his fellow-worker in

Quaternions—Professor Tait. The record in his journal of this congenial intercourse shows that it was much enjoyed by him. The books in preparation by both on their common subject—Quaternions—were well talked over. His journal of the first day records: 'I walked with him nearly to the foot of the lane. At parting he wanted to be quite sure what my own wishes were on the subject of our respective publications. I said that they could literally be expressed in *two words*—"Sixty-one, Sixty-two:" meaning of course that *I* should have 186*1* free to myself, and that he might have 186*2* to do what he liked in.' In a letter of 1862 (August 29), Hamilton writes to Tait:—'You are perfectly at liberty to refer [in an intended contribution to a scientific periodical] in any manner you choose to my forthcoming volume, and generally I have entire confidence in your discretion. The only thing I asked was that you would not publish a separate work before the appearance of the *Elements*. I shall be charmed, for *both* our sakes, to set you free as soon as possible.'

This, and other similar statements, show how little able was Hamilton to forecast the time which his great work would occupy, or the extent to which it would proceed. I have already recorded that at first he imagined it would not exceed 400 pages, and now we see he felt sure it would be completed within the year 1861—he had previously named a much earlier date—whereas, in fact, the work extended to nearly 800 pages, and was left incomplete at his death in the autumn of 1865. His mistakes were attributable not to any want of industrious perseverance, which in him was habitual, but to the vastness of the scale upon which it was natural to him to work, and to his inability to calculate the extent of time which would be required for the mechanical transference into manuscript and printer's type of the mathematical operations and conclusions which were already mentally conceived with perfected grasp and distinctness.

I think it only just that I should here frankly state that a perusal of the correspondence between Hamilton and Tait has left on my mind, first, a conviction that, in this matter of their

projected works on Quaternions, Professor Tait acted throughout with strict regard to honourable obligation, and, secondly, a corresponding impression that Hamilton, in desiring his scholar to withhold the publication of his *Examples in Quaternions* until after the appearance of the *Elements*, may have fallen into an error, though a most natural error, of judgment. Besides miscalculating the time which the completion of his own work would require, Hamilton naturally felt it to be scarcely fair to himself that one to whom he had freely communicated unpublished methods and results should precede him in making them known to the scientific world; and, looking beyond himself, he felt desirous to secure for his own University the credit which would attach to the first publication of a methodical treatise on Quaternions. It may, however, I think, be believed that, had he not been too modest to estimate fully the magnitude of his own undertaking, he would have been aware of its superiority to all possible rivalry, and would have arrived at the conclusion that the earlier publication of Professor Tait's *Examples* would have tended, not to injure, but to promote the success of the *Elements*, by keeping alive an interest in the subject, and by preparing students for a proper comprehension of the *magnum opus* that was to follow. Professor Tait's work was published, after that of his master, in 1867, and an enlarged edition, in producing which he enjoyed the co-operation of Professor Kelland, in 1873. This edition bore the title of *Introduction to Quaternions, with numerous Examples.*

The following month of July was rendered notable to Hamilton by the appearance of the great Comet of the year, first seen by his son Archibald, amongst its earliest observers, on Sunday, the 30th of June, and then on succeeding nights watched for by himself.

He notes, *July* 4, 1861:—'It is certainly a very fine one and probably much superior to Donati's [1858], although I have not yet seen it favourably. But I was greatly struck by the brightness of its nucleus, *as seen through clouds*, at 2 o'clock on ☿ morning.'

On the 10th of the month the Astronomer himself, in an environment unusual to him, appeared before the public as presiding over a lecture, given at the Rotunda in Dublin, upon Japan by Dr. Mac Gowan, who had been a missionary in Japan and China, when these countries were less known than they now are, and whom Hamilton had met in 1859 as one of the guests at Banchory House, near Aberdeen. Hamilton also, in the succeeding month, took some part in the public meetings, over which Lord Brougham presided, of the Social Science Congress. And he had the gratification of introducing to Lord Brougham his second son as a student in physical science, who had manifested original thought in investigations connected with earth-currents of electricity. But the greatest gain brought to him by these meetings was his friendship, then first entered upon, with Dr. Charles Mansfield Ingleby, who, taking advantage of his visit to the Congress, sought to be allowed to make the acquaintance of one whom he said that Englishmen regarded as the greatest of living Irishmen. Though accomplished in both branches of science, it was as a metaphysician rather than as a mathematician that Dr. Ingleby approached Hamilton. Their day of meeting was occupied with discussions on Kant and his philosophy—discussions which led to a correspondence from which some extracts will be given. Shortly afterwards, contended for as a guest by friends new and old, and this time received by Mr. Oliver Heywood, Hamilton attended the Meeting of the British Association at Manchester, and on his return visited, at Croft Rectory, near Warrington, a brother mathematician, Thomas Pennington Kirkman, a graduate of Trinity College, Dublin, already favourably known to him by correspondence. That Mr. Kirkman was a labourer of thoroughness and perseverance akin to Hamilton's own may be inferred from the following passage in a letter written by him to Hamilton in 1862. His field of investigation was 'polyhedra,' the same which had yielded to Hamilton his Icosian Calculus. Speaking of his results, Mr. Kirkman writes :—

'*July* 15, 1862.—The labour you may judge of when I say

that the calculations cannot be written out in fewer than 800 close quarto pages. I have 500 pages of reticulations written, namely, the preparatory list of groups of reticulations that can be made out of nine particularized polygons charged with polyhedra of nine edges and under; and I have not by a great deal completed even these, in which I do not go beyond constructions of twenty-four edges.'

The following extracts will prove to the reader what were the estimates formed by these men of each other. Kirkman writes:—

'*July* 15, 1862.—I wish I had the good fortune to be nearer to such a mathematician as you; for it would be of immense advantage to have the profit of conversation with such a man. It is a great loss to me to live cut off from all scientific intercourse.'

Hamilton to Kirkman [from a draft], *July* 28, 1862. . . . 'It would be very difficult for me to express, without having the air of flattering, how much I admire your mathematical genius and discoveries; but it is a real pleasure to me to be allowed any little opportunities, such as these, of testifying my respect and good-will. Some time or other I hope that you and Mrs. Kirkman may be guests of Lady Hamilton and myself here. But it is only fair to apprise you that my wife, like yours, uses an "Arabian" for herself, and that we have no carriage, &c. In short, I lead a very retired life; which, as I grow older, I devote more and more to study. I might now find a difficulty in getting up a dinner-party for you, though I have given a good many in my time. Perhaps I may just be allowed to add that I am conscious of not having been in my normal state of health during the whole of my last visit to Manchester. . . .'

What Hamilton meant by devotion of life to study is indicated by facts which I find incidentally recorded, that on one day in August, 1861, he spent 'more than twelve hours at work,' and on the 29th of October following 'at least thirteen consecutive hours.'

At the end of the year Hamilton indulged himself with the purchase of *Love the Greatest Enchantment, &c.*, from the Spanish of Calderon, by D. F. Mac Carthy. In perusing this translation,

specially commendable for its adherence to the metre of the original, Hamilton came upon a passage expressing the connexion between philosophy and mathematics, which as a philosophical mathematician he could not but challenge as a mistaken interpretation. He thus, with characteristic deference, submits the point to the translator, whose reply was a frank admission of the blot hit by his friendly critic.

From SIR W. R. HAMILTON *to* DENIS FLORENCE MAC CARTHY.

'OBSERVATORY, *December* 13, 1861.

'You have now a great opportunity of instructing me at once in Mathematics, Philosophy, and Spanish.

'At p. 43 occurs the passage

"No te digo, que estudié
Con generoso motivo
Matemáticas, de quien
La filosofía principio
Fué; no te digo, que al cielo
Los dos movimientos mido,
Natural y rapto, . . ."

which of course increases my intellectual respect for the Enchantress of ancient fable. I suspect that Ulysses hardly caught her allusion to the distinction between *natural* and *violent* motion; but, waiving Astronomy, and all old speculations on physical science, I want to ask you whether Circe may not (through her interpreter, Calderon) have spoken of Philosophy as the *base* or "principio" of Mathematics, rather than the latter of the former? . . .'

CHAPTER XLVII.

EXPENSE OF PRINTING. RÖBER'S ESSAY. AMOUNT OF SCRIBE-TOIL.

(1862.)

At this time, when his *Elements* were approaching the five-hundreth page, Hamilton was brought into a state of serious anxiety by the exhaustion of the grant which the Board of Trinity College had made towards defraying the cost of printing. He saw that he was threatened with an expenditure which his private resources were insufficient to meet. In this crisis he imparted his difficulty to his friend Dr. Hart, from whom in return he received a suggestion that, following the successful example of Dr. Salmon, he should try whether an English publisher would not share with him the risk of publication. From letters of Hamilton written to Dr. Hart on this subject I make some extracts which will be found not only to unveil the extent of his anxieties and his labours, but incidentally to express in memorable terms what were his feelings towards Dr. Salmon, and what were his convictions in regard to the work he had in hand, and to the need that, in using and carrying forward his discovery, he should have the aid of other labourers in the field of the Higher Algebra.

From Sir W. R. Hamilton *to* A. S. Hart, ll.d., s.f.t.c.d.

'*May* 17, 1862.

'. . . To you I shall then send a fair and final copy of that half-sheet, but when I may have another half-sheet of the *Elements* to send you is more than I can say. For the crisis is come at last, and the impression is suspended for want of funds. I have not yet made any formal application to the Board, much less to the

Royal Society, or any other quarter, but have forwarded all the pages to my old friend, Dr. Lloyd, who, as you told me, acts now as Registrar. Faithfully yours.'

'P. S.—I know that you are not the officer of the College to be troubled just now with any application; but since we have been lately in free communication with each other, I shall just say that I think the Board would do themselves honour, and serve the cause of Science, by voting a new (and final) grant of £100, to make possible the publication of the *Elements*. It is all fair that I should run *some risk*—say even £50—as probably I *shall* after *that;* but has Ireland ever produced a *new branch of Mathematics*, or, say, only a *new Calculus* before? And can you or anyone else really suppose that I can enter the *market* on any terms of equality with our friend Salmon, whom I profoundly admire and *love*—but who finds *his public* ready made, namely, all the mathematicians of the living world: whereas I have to *create* my public? In short, can you seriously suppose that the publication of the *Elements* is a *commercial* enterprise?'

'*May* 24, 1862. . . . The chief feature of the whole work—whenever it can be printed and published—will probably be that *Theory of Linear Functions* of Vectors and Quaternions, to which I lately alluded. As you have been so good as to seem to take some interest in it, and in the work generally, I have thought that you might not dislike my making up for you what I consider as my *final* copy of the first six articles, with their sub-articles, of the sixth section of the last chapter. You need not be in any haste to return it, at least until it can be printed, and you can let any friend in College see it if you choose. It will show, I think, that great pains have been taken to produce clearness and *compression*. Yet I fear that the copy sent represents only about a third part of the section. After *it*, however, there will remain only *applications*, easier by far, and which it will be the distinctive mark of *success* to be able to treat *briefly*. Besides, Application enters only subordinately into the conception of my present work. I want to finish a *Book of Reference*—a *short* one, unluckily for sale, it cannot now be—but my intention for myself, and *hope* as regards other writers, is that the *Elements* may be *cited*, almost like the στοιχεῖα of Euclid, in future treatises or memoirs on the Quaternions. My own hope is to cite them in future essays in the *Transactions* of the Royal Irish Academy, &c.'

'*May* 27, 1862. . . . I have wished to submit to the Board, previously to applying for their aid, the manuscript of that sixth section of the final chapter on Linear Functions of Vectors, &c., which will be, as I expect, the principal feature of the volume, and of which the geometrical, and, above all, the *physical* applications appear likely to be of vast importance—in the hands of *other persons*. . . . It would be rash to *promise* that the volume shall be concluded under 500 pages—at least if *some* contents and *some* preface be included—but I assure you that I am beginning to *wish* to have done with it. The labour has been very severe, including the mechanical part, which, however, appeared to be necessary. I estimate that the work will have cost me not less than *ten thousand hours*. Let us call it the *advance* (and not the *ἀνάβασις*) of the Ten Thousand.'

On the 14th of June his old friend Dr. Lloyd had, as acting Registrar, the gratification of communicating to Hamilton the consent of the Board to advance a second £100 towards the printing of the *Elements*, accompanied, however, by the conditions that Hamilton was, as he himself had proposed, 'to pay the remainder of the cost, and that the foregoing sum was to be paid by the Bursar when the work was completed.' In returning his best thanks through Lloyd to the Board, Hamilton adds:—'It will make the publication of the *Elements of Quaternions* in a satisfactory form *possible*; and it is my business to look to the rest. It will protect me from actual pecuniary *loss*, and that is about as much as I look for—except in the way of fame for the College and myself.'

It must here be noticed, what the reader may have anticipated, that the continually increasing size of his book inevitably renewed the anxieties which I have been recording, and which were not to cease before his death.

The following extract from a letter to myself (May 20, 1862) proves that, notwithstanding these anxieties, he was at this time working with unremitting zeal, and places on record his judgment as to the comparative merits of his two great volumes on Quaternions:—'. . . I am still intensely occupied with my new work, the

Elements of Quaternions, which I consider to be incomparably superior as a *book* to the *Lectures*, although the earlier one may perhaps be considered to possess a greater interest in the History of Science.'

In the autumn of 1862 Hamilton's work on the *Elements* was agreeably interrupted by a scientific investigation of a completely different character. The Archbishop of Dublin, Dr. Whately, had encouraged his son-in-law, Mr. C. B. Wale, to act upon the wish of a friend by requesting Hamilton's opinion on two essays by Friedrich Gottlob Röber, the object of the first of which was to set forth a geometrical construction of the regular heptagon.

The circumstances and the result rendered this a very interesting investigation, for the supposed geometrical discovery announced had been arrived at by Röber, who was Professor of Architecture at Dresden, from considering the measurements of an Egyptian Temple—that of Edfu on the Nile. And so much of interest still attaches to the question of the extent and purpose of the use of geometrical knowledge in the structure of Egyptian Temples and Pyramids, that I think it right here to insert two of Hamilton's original records of his investigation.*

It is true that his results were published in the *Philosophical Magazine* for February, 1864; but my reader will, I doubt not, enjoy the perusal of documents written when the impressions made upon his mind by the study of this interesting subject were fresh and deep. I reproduce, first, Hamilton's *précis* of the facts, as I find it in *Manuscript Book* B., 1845, p. 121, and, next, his letter to Mr. Wale, which gives glowing expression to his feelings, as well as a pointed statement of his results.

* In discussing the question to which I have above referred it has, I think, to be borne in mind that although these Egyptian edifices may have been, as to their main objects, religious or memorial, their architects undeniably exhibited in the construction of them a notable amount of geometrical knowledge, and may not unreasonably have been supposed to have adopted means of indicating and so conserving the truths upon which they worked.

'On Röber's Construction of the Heptagon.

'1. On *September* 11, 1862, Mr. C. R. Wale, son-in-law of the Archbishop of Dublin, drove out to this Observatory, but did not come in: he left, however, a note to me, and also a couple of German Essays, by Friedrich Röber, which had been sent through Mr. Wale, from Mr. Sharman, of Montreux, Canton de Vaux, Suisse, with a request that I should examine them, and give my opinion on their accuracy and value, or, at least generally, send some message by him to his friends in Switzerland, on his return.

'2. The earliest of the two Essays is entitled: *Beiträge zur Erforschung der Geometrischen Grundformen in den alten Templen Aegyptens, und deren Beziehung zur alten Naturerkenntniss, von* Friedrich Röber. Mit IV *Lithographirten Tafeln. Dresden. Verlag von Voldemar Türk*, 1854.

'3. I understand the author to say, in his preface, that his deceased father, who seems to have been Professor of Architecture in the Academy of Dresden, and to have died at Paris in 1833, conceived himself to have explained the construction of the ancient *Temple of Edfu*, as being connected with the *Inscription of a Regular Heptagon* in a circle, on a plan which he had discovered.

'4. Accordingly, at page 15 of the Memoir or Essay, he gives —but I take it as a process discovered by his father—a geometrical construction for the following problem: "Einen gleichschenkligen Triangel zu beschreiben in welchem jeder der Winkel an der Grundlinie das Dreifache des dritten Winkel ist." And the *construction* is illustrated by a *Diagram*, Tab. I. *Fig.* 1., which is not of excessive complexity.

'5. My first impression was that I should find either an *error*, or at least a *rude* approximation; but to my great surprise, indeed astonishment, on repeating all the calculations, with *Taylor's Logarithms*, I found myself unable to decide whether the result erred in *excess*, or in *defect:* assuming that it *must err*, because only *right lines* and *circles* are employed, while the problem depends essentially on the solution of a *cubic equation*, and therefore *cannot* be resolved by extraction of *square roots* alone.

'6. And I wrote a note, dated September 12th, to *Professor De Morgan* to express this feeling of surprise: giving at the same time the *formula* by which I had *expressed* the construction.

'7. Finding that *seven-figure logarithms* left the question doubtful—at least as I had used them—I resolved to go through the entire calculation by *arithmetic* alone, and without any tables.

'8. I have since performed the calculation with still greater accuracy than before: *aiming* indeed at having 15 decimal places correct, and using often 16 decimals in the calculations. Contenting myself, however, with 13 in the *final result*, I find that *Röber's construction* gives

$$\zeta - 1 = \cos\frac{2\pi}{7} = 0{\cdot}62349\ 00759\ 241\,;$$

while the *cubic equation* $2x^3 + x^2 - x = \frac{1}{4}$ gives

$$x = \cos\frac{2\pi}{7} = 0{\cdot}62348\ 98018\ 587.$$

'9. Röber's *cosine* is therefore *greater* than the true one, by about 247 *in the 9th place* of decimals, which answers to a *defect of arc*, amounting to about 35 *in the 8th place*, if the *radius* be taken for *unity* and consequently if a *tenth of a second* be represented by about 5 *in the 7th place* of decimals. *His error* then, on a *single seventh of the circumference*, is only about $-\,0''{\cdot}07$; and the *error of his sevenfold arc* is a *defect*, but one which *scarcely amounts to half a second:* or that *sevenfold arc* is indeed *shorter than the true circumference*, but only by about the 800,000*th part of the diameter.*

'10. On the *equator of the earth* the error of a *single seventh* would only be about *seven feet.* If the *radius* were *fifty feet*, or 600 inches, the error of the *single arc* would be only *three halves* of the thousandth part of an inch, and therefore scarcely visible, if at all, to the naked eye. *On the great circle of this Observatory*, the *sevenfold arc* would err by only about the *eight thousandth part of an inch.*'

From Sir W. R. Hamilton *to* C. B. Wale, Esq.

'Observatory, *September* 15, 1862.

'A wish to gratify the archbishop and yourself was the first motive for my attempting to examine to some small extent the Essays of Röber which you had the goodness to leave for me a few days ago, and to form some opinion on their value, unimportant as that opinion might be. But the Memoir on the ancient temples of Egypt (Röber, Dresden, 1854) has interested me

profoundly. Indeed I have scarcely been able, since I opened it, to attend to anything else; and it led me into some long calculations which I have only just completed to my satisfaction. As I have paid no special attention to Egyptian Antiquities, nor meditated much on such mystical guesses as some have made at their inner meaning, the only point which I could hope to study usefully was the *geometrical discovery* announced in the first memoir, namely, "the construction of the regular heptagon," which the elder Röber appears to have *divined*, from the study of the ancient Temple Architecture.*

'I entered on the subject, perhaps with prejudice; for like most (if not all) modern geometers, I have been accustomed to hold, and indeed still do hold, that it is *impossible* to construct such a heptagon with the "right line" and "circle" *alone*. Yet, to my great surprise, I found no error in Röber's *numbers;* and on repeating the calculations on another plan, with Taylor's seven-figure logarithms, I found myself quite unable to pronounce whether Röber's *arc* erred in excess or in defect from the *exact* seventh part of the circumference; for that it *must err* I felt assured.

'It seemed, therefore, worth while to go much more closely to work; and laying tables entirely aside, to perform the *whole* of the work for myself, by *arithmetic alone*, and especially by extractions of *square roots*. And to be quite sure of a high degree of accuracy in the final result, I made it a rule to work with not fewer than *fifteen decimal places*, besides employing all verifications that I could think of in the progress of calculation, which, thus laboriously conducted, has covered many sheets of paper, and cost me many hours on two or three successive days.

'At last, however, it is finished; and I should have no hesitation to commit myself publicly to the result, which is, technically expressed, that the natural *cosines* of the angle assigned by Röber's construction is, to thirteen decimals, 0·62349 00759 241, whereas the true cosine of the seventh part of four right angles deduced to a corresponding accuracy from a known cubic equation I find to be *a little less*, namely, 0·62348 98018 587. Admitting, though I do not believe it, that the two or three last of these decimals *may*

* The subject of the Second Essay was The Pyramids and Parthenon.

be wrong after all the precautions taken, I am quite satisfied that the cosine of the *Egyptian Angle*—for really Röber seems to make it likely that the Egyptians did employ it—is *somewhat greater*, and that the cosine of the *true* or *geometrical* angle (for of course we can in geometrical *conception* divide the circumference into *any number* of equal parts) is somewhat less than 0·62349, and consequently that the supposed *Egyptian rule* of the *heptagon* is *not mathematically perfect*, though Röber seems to suppose it to be so. And if you should ever think me worth citing on the subject, I request you to bear in mind that such is *one* of the results of my investigation.

'But now let us turn the tables and inquire *how near* does the supposed ancient rule come to the truth? *How small*, in *practice*, is the *error* which *theory* pronounces to *exist?* And I answer that in *practice* the *error* does not exist *at all.* I do not think that *experiments* of measurement, &c., *could* be so conducted by men, at least in the present age, as to prove to sight that there was any error. For *practical purposes*, then, the elder of the Röbers, or the old Egyptian sage whose secrets he supposed himself to have divined, has *done the impossible.*

'We have in this (Dublin) Observatory a circle movable in azimuth, the largest of the kind in the world. If on this *Great Circle* (and of which the radius is four feet) seven successive Egyptian arcs were set off from an assumed *zero* or initial point, the last would end within about the *eight-thousandth part of an inch* of that point with which the *first began;* and on the *earth's equator* this *sevenfold error*, which is one of defect, would only amount to about 50 feet. In short I compute that the *sevenfold* arc falls *short* of the true circumference, but only by about *half a second.* And although we *aim* at *not neglecting half* or even *the tenth part* of a second in astronomy, yet such an error of observation very often occurs, and nobody could be sure of detecting the eight-thousandth of an inch, so as to say that it had not arisen from some slight *slip* of the compasses, &c., in the construction. But calculation carefully conducted is infallible, and I have no shadow of doubt upon the question.

'Yet the *practical success* of the rule is to me absolutely *wonderful:* and it is long since any discovery in science produced in me such a sensation of *surprise.* It enables me more than before

to realize what we are told of the "wisdom of the Egyptians." Perhaps the construction was disclosed to Moses—though one cannot see what use it can have been to him, except perhaps as an element of intellectual preparation. Some think that Euclid—who certainly knew much more than what was in his *Elements*—derived instruction from the East or from Egypt. If so, and if the rule was at that time known there, according to the Theory of Röber, I do not think it necessary to suppose that Euclid refrained, from any oath or obligation of secrecy, from publishing the construction of the Heptagon. He was not in the habit of *believing his eyes* in Geometry. It was nothing to him that the construction was reasonably simple, and that he could *see* no want of "closing up" when the sevenfold arc had been completed. He doubtless asked for a *proof*: and the Egyptians had none to give.* Such then are my two main conclusions, which I may thus recapitulate:

'(1) The (alleged) *Egyptian Rule* for the construction of the Regular Heptagon is, in rigour of theory, erroneous.

'(2) The *same rule* of construction of the Heptagon is, however, for *all practical purposes, perfect.*

'No artist of the present day, I feel sure, would undertake to divide a circle into 7 (seven) equal parts, with a superior, or even an *equal* accuracy, to that which the construction, if fully carried out, would give.

'I beg that, when you have the opportunity, you will convey my best thanks to Mr. Sharman of Montreux for his very welcome present of those two Memoirs of F. Röber. And I hope that by the labour which I have recently gone through, on the occasion of reading one of them, and by the length to which this letter has extended, I have sufficiently marked my thanks to you, and my affectionate respect for the Archbishop. . . .'

* Hamilton here gives to Euclid a character which belonged also to himself. I remember a comment of his upon a fact mentioned in a letter from Dr. Haughton, S.F.T.C.D., now President of the Royal Irish Academy—a man in regard to whom I have met with a strong expression of Hamilton's 'respect and admiration.' That letter contained the following sentences:—'I know of no person who has not seen Conical Refraction that really believed in it. I have myself converted at least a *score* of mathematicians, by showing them the *Cone of Light.*' Hamilton's laughing remark on the persons referred to by Dr. Haughton was this:—'How different from me! If I had *seen* it only, I should not have believed it. My eyes have too often deceived me. I believe it, because I have proved it.'

In carrying through this investigation he found it convenient to employ what he calls* 'that powerful method of Horner, for the arithmetical calculation of numerical equations,' a method he had long known theoretically, but had not practised.† Of this method Professor De Morgan was an intense admirer, considering it the greatest *recorded* discovery in arithmetic. He furnished Hamilton with some improvements in the manner of working with it, and in a sort of rivalry they both proceeded to calculate to 22 places of decimals the cos. of $\frac{2\pi}{7}$, which, working independently, they found to be

$$= 0{\cdot}62348\ 98018\ 58733\ 53052\ 50.$$

Hamilton's manuscript books afford striking evidence of the amount of numerical computation requisite to arrive at such a result: they show also that he was not content with working upon the particular problem before him, but that other cubic equations furnished him with material to which he vigorously applied the same method: in one case carrying out his calculation to 28 places of decimals. Yet this laborious work was in a sense play to him. On the 17th of September he writes to De Morgan:—'All this has quite distracted me from the *Elements*.' On the 26th he writes:—'I must now drop arithmetic for awhile; though the work gone through has rather *refreshed* than fatigued me.' In the autumn of the following year he again corresponded with De Morgan on this subject, and prepared the memoir respecting it, which was printed in the *Philosophical Magazine* for February, 1864—his last contribution to that periodical.

It may here be mentioned that at this time Hamilton was a very solitary labourer. Except what the Observatory assistant, Mr. Thompson, could but rarely render him, he had no help in

* *New Method of solving Numerical Equations*. By W. G. Horner: London, 1819.

† See Manuscript Book, '1863, Number 6,' p. 21, draft letter to his son Archibald.

keeping copies of mathematical work or correspondence. His manuscript books had some years previously borne witness to the assistance dutifully rendered to him in this way by his two sons; but now both of them had left their home. His eldest son had, in 1862, accompanied his Aunt Sydney, his father's only surviving sister, to Nicaragua, where for some years she devoted herself to keeping a school for girls at Greytown,* and where he sought for employment; and the younger son, who, for some time after his ordination in 1860, had served as curate of his native parish, Castleknock, and while so acting resided at home, had now entered into clerical engagements at a distance. Occasionally, indeed, the feminine hand of his daughter appears in those manuscript books, requisitioned on an emergency to copy a letter or even a scientific paper, but usually the labour is all his own, and it must have been enormous. Few persons, it is true, would have kept such full record of correspondence, and all particulars connected with it, as Hamilton was in the habit of doing, but still the great mass of this voluminous material was genuine and important work. And it causes to the reader who examines it a perpetually renewed feeling of astonishment at the amount of industry it represents.

* It appears, from an interesting and able letter from her to her brother, that Sydney Hamilton at this time desired much the rehabilitation of the Observatory at Bogotá in Columbia (of which the site was the nearest to the equator, and at the highest altitude of any Observatory in the world), and even entertained the idea of offering her own services towards the undertaking, as accustomed to observe, and to make reductions of Astronomical observations.

CHAPTER XLVIII.

SCIENTIFIC ITEMS. CONCLUSION OF DE VERE CORRESPONDENCE. THE ROYAL IRISH ACADEMY AND THE ROYAL DUBLIN SOCIETY.

(1863.)

THE year of Hamilton's life at which we have arrived was marked by no outward events in his history, yet the reader will, I think, find interest in some extracts which I proceed to give from letters which passed about this time between Hamilton and various scientific friends. They will be followed by the concluding portion of the correspondence with Aubrey De Vere. At the end of the chapter will be found Hamilton's '*Memoranda*' of the part taken by him in an important discussion at the Royal Irish Academy on a proposed subordination of the Academy to the Royal Dublin Society.

From PROFESSOR P. G. TAIT *to* SIR W. R. HAMILTON.

'*April* 3, 1862.

'I showed Conical Refraction to twenty of my class this morning (at once), having splendid sunshine.'

'*August* 26, 1862.—Professor Bolzani is here and we have had long discussions on the subject [Quaternions]. He is immensely interested in it, and has given a course of lectures on it in Russia, of which he has promised to send copies when printed. He intends to visit you in Dublin before returning to Kasan.'

After urging Hamilton further to develop particular results, which he indicates, Tait continues:—

'Such things as these would show the enormous value of quaternions to *Physicists*, and that is in my eyes worth any amount of pure analysis or geometrical theorems.'

Hamilton to Professor Tait, acknowledging the receipt of a *proof* of Chap. II. of Thomson and Tait's *Dynamical Laws and Principles*, writes:—

'I am much obliged, and expect to *learn much* from a careful perusal of the Treatise. And indeed I have much to learn.

'The world of Science seems to admit that I had not only read but written to some purpose on *Dynamics* about *thirty years ago*; but a new generation has arisen. *Energy* and *Work*, in the *old* English meaning, are things not unfamiliar to me. But I have only the dimmest views of the modern meanings attached to those terms. From "Thomson and Tait" I hope at last to be instructed on the point: *not*, of course, without patient study on my part. A certain humility and teachableness of the *ethical* kind I can promise.

'I have long got so far as to understand the *foot-pound* as applied to the *raising* of weights. But if 10,000 foot-pounds of work have been expended in raising 100 pounds through 100 feet, and if the load be then *let fall*, and descend to its first level, I wish to be instructed whether and why and in what sense you Energists deny that a *negative myriad* of foot-pounds of *work* has been done by *gravity*. When poor Sisyphus had patiently toiled in rolling his great stone up the hill, and back again

κυλίνδετο λᾶας ἀναιδής,

(I quote from the Odyssey *memoriter*, and probably inaccurately) might he not have consoled himself by the thought that the avenging Fates, or Furies, had been compelled to take some counteracting trouble? Your book when complete, may enable *even me* to understand all this, slight as my *instinct* is for physics.'

The same disposition to appreciate fully the work of others and to submit himself as a learner, while at the same time claiming the share of credit due to himself, is manifested in the following acknowledgment to its publishers of a presentation copy of a scientific treatise.

To MESSRS. MACMILLAN & Co.

'*April* 10, 1863.

'I am much obliged by the attention on the part of the authors, and of yourselves, in your having lately sent me a complete copy of the obviously very valuable *Treatise on Solid Geometry* by Messrs. Frost & Wolstenholme. . . . I have already treated, by my own calculus, a good many rather difficult questions of solid geometry; for example, one which is analogous to, but greatly more general than, that of the *Edge of a Tubular Surface*, which I see treated at page 373 of the Treatise recently sent me. In doing so I integrate and interpret, by quaternions, a differential equation of the second order and second degree, between four variables, which was assigned by Monge in his *Application de l'Analyse à la Géométrie* (page 372 of Liouville's Edition, or page 325 of my old friend the Fourth Edition, Paris, 1809, with which I was familiar when a boy), but which was left *un*integrated and *un*interpreted, by the illustrious author of that great work. There will therefore be a vast deal to *interest* me in the new publication which you have forwarded, and if it have arrived too late for me to *learn* much from it *just now*, I hope at least to make an honourable *mention* of it, in any preface, however short, which I may prefix to my own work, for the publishing of which no *arrangements* have yet been made. I am, dear sirs, requesting you to present my regards to the authors, your obedient servant.'

Throughout the year 1863 Hamilton was engaged in the investigation by his own methods of osculating twisted cubics and in the discussion of a general centre of applied forces (subjects on which he made communications to the Royal Irish Academy, which were printed in its *Proceedings*, April 27, 1863, and June 22, 1863), and he made frequent inquiries by letters to Dr. Salmon and Dr Hart as to whether his results had been anticipated. A postcript to one of Dr Salmon's replies was calculated to give him encouragement.

'*October* 1, 1863.—You so seldom fall on other peoples' track, that when I met Hart a couple of days ago, and he told me you had been asking him whether certain theorems you had arrived at were new, I said: "Oh, tell him they are new. You will be quite safe."'

In a letter of October 3, 1863, to his son Archibald (now, it will be remembered, a clergyman), who had been making some algebraical calculations, and sought for information respecting them, he supplies him with the computation of $\epsilon^{\frac{1}{2}}$ ['$\epsilon = 2{\cdot}71828\ldots$ = the Napierian, or *natural*, base of logarithms'] by the exponential series, and with its verification by squaring, in which he used a kind of contracted multiplication, which, he says, he picked up from his assistant, Mr. Thompson. The following passage has a value as evidencing the pains he habitually took to be accurate, and as conveying his advice on the point to his son:—

'I always verify the multiplications by divisions mentally, and add both upwards and downwards, often making a note of the figure carried to the next left-hand column. In general, I take all precautions for accuracy, and advise you to do the same. Whatever you *do* calculate—and you know you *need* not do so more than you choose—let it be *carefully* done.'

Then after speaking of Horner's method, in terms which I have already quoted,* he adds:—

'I shall gladly get you somewhat larger tables if you have any real occasion for them, only don't spend *too much* time on such things. *How much* can be spared you can judge for yourself. I know that a complete change of mental occupation is occasionally useful and almost necessary. My *book* will get on none the worse for my having been lately thus working at *arithmetic*.'

His occupation with arithmetic was now unexpectedly prolonged by his neighbour, Mr. J. G. Rathborne, submitting to him a very meritorious system of contracted multiplication which

* *Supra*, p. 147.

had been devised by a self-taught man in his employment, named Boyers. Hamilton considered it to be 'new and ingenious.' 'It had,' in comparison with a method ordinarily used by himself, 'the advantage (he wrote, October 19, 1863) of giving a succession of approximations, each better than the preceding, but required rather more of mental calculation in passing from step to step, yet not more than would be found easy by a practised calculator.'

I may conclude these extracts with one from a letter written early in the year by the eldest son of Sir John Herschel. He had paid a visit at the Observatory, and on his return home, in thanking for a letter of his father forwarded to him, quotes from it the following message relative to Hamilton's book:—

'When it is complete, as I hope it soon will be, I shall take it up *ab initio*, though without great hope of mastering it in the few years that remain to me after 72! But I anticipate for him (Sir William) a real triumph in its publication.'

I now present to the reader the concluding portion of Hamilton's correspondence with Aubrey De Vere, a correspondence which, in my estimation, constitutes an integral and most valuable element in his life. I consider it a privilege to have been allowed to give it, even imperfectly, to the world, and am confident that no reader of this biography would willingly lose the share contributed to it by Hamilton's friend. This friend paid him a visit at the Observatory in the early part of 1863. In this winter Hamilton was troubled by that inherited malady of gout, by which, as we have seen, he was visited at Cheltenham in 1856, which had subsequently recurred at intervals, and which was ere long to prove fatal to him. The first of the following letters refers to this disturbance of his health, and leads the writer to make interesting record of his experience in relation to the effect of bodily pain or illness upon the action of his intellect:—

From Sir W. R. Hamilton *to* Aubrey De Vere.

'Observatory, *February* 9, 1863.

'I enjoyed very much your visit, and my walk with you, and you may perhaps be glad to know that though (from want of recent practice) I felt since some stiffness in my legs, the feet have been quite uninjured, or, more plainly, no return of gout has been provoked.

'I wish you would try a fit. For one's philosophy and temper it is an admirable exercise; and to some constitutions it seems to be *useful*. I had been *wearying* for a slight touch again, when I was taken at my word last Christmas.

'My intellect, such as it is, appears to me to be made even *clearer* by sickness or bodily weakness, when such comes. The physical effect, however, is, that although I can scarcely spare more than a moment to *attend* to a twinge of the gout—painful as that certainly is—I cannot *work*, when an invalid, for so many hours consecutively, as when in full and normal health. A feeling of *fatigue* comes on.

'But I call myself quite well now, thanks partly to your visit, and remain, my dear Aubrey, your old and affectionate friend.'

The note to which the following is a reply has not come into my possession:—

From Aubrey De Vere *to* Sir W. R. Hamilton.

'12, Leinster-street, *March* 6, 1863.

'... I have also a note to thank you for, parts of which—relating to our conversation when we had our walk—touched me very much. I can well understand how deeply you must have felt, being thus visited once more as by a hand stretched out from your boyish days. The heart never grows old—if it ever was young; and sometimes clings the more to the feelings that belong to its youth, when youth as counted by years is leaving us. I have given a reason for this in one of my poems called "Psyche," page 38. Affectionately yours.'

The last letter written by Hamilton to Aubrey De Vere was worthy of him as a friend and as a Christian; as a friend, who,

notwithstanding that serious differences of opinion had arisen between them, could still continue to love and trust his friend; as a Christian who, raised above minor details, could rejoice to hold fast, in sympathy with his friend, the cardinal truths of his religion. The last letter of Aubrey De Vere to Hamilton is, I think, one of singular beauty, both in the vision it calls up of the days that had gone by, and in the perfection with which thoughts of tender sadness and affectionate reminiscence are expressed. It binds together the early meeting of the two friends at Curragh Chase—where Lord Adare and the parents and sister of Mr. De Vere completed the party, and where social intercourse was enjoyed which was never forgotten by either—with the days, now more than forty years afterwards, when their last words, words of faithful affection and Christian harmony, had come to be exchanged.

From Sir W. R. Hamilton *to* Aubrey De Vere.

'Observatory, *April* 3, 1863.
'*Good Friday.*

'My dear Aubrey—You may not think this *day* a *good* one for writing a letter even to an old friend; but I have just been reading again your *Hymn for Good Friday*, both in the edition of 1842, and in the poems published in 1855. Allow me to say, in passing, that if there be *a word or two*, in the latter edition, which you could not expect me to adopt, I yet admit that on the whole you have made it a *finer poem.*

'It may be more important to remark that I humbly conceive myself to be as much a Catholic *as you were*, when you wrote the hymn in its *first form;* and it is a *comfort*, in these Colenso days, to have an opportunity of refreshing, by a reperusal of it, a sympathy so sincere in the most *vital* doctrines of that Christianity, which we both profess to believe:—

"O Lamb of God! on whom alone
Earth's penal weight of sin was thrown,
Have mercy, Saviour, on Thine own!
For thou art Man. The Virgin gave
To Thee her breast, the earth a grave."

"O Lamb of God, on whom was laid
The debt all worlds had never paid,
Have mercy, Saviour; hear and aid.
For thou art God. . . ."

"Thus, Christ, we turn from all to Thee;
Miserere, Domine."

'The "For thou art Man," and the "For thou art God," and the closing "Miserere, Domine," appear to be the *most practical teachings* of the Christian Religion: or let me say, with greater reverence, *among* the most, if it be presumptuous and hazardous to distinguish. . . . I remain, my dear Aubrey, your old and affectionate friend.'

From AUBREY DE VERE *to* SIR W. R. HAMILTON.

'CURRAGH CHASE, ADARE, *May* 14, 1863.

'MY DEAR HAMILTON—I have not been as ungrateful as I may perhaps have seemed, in not having before now answered your letter written on Good Friday. . . .

'Here I am, once more, after an absence of about two years, in my old home, and alone; for my eldest brother and his wife are in England. It is to me haunted ground. After a time, of course, this effect wears off; but at first after coming here, it really seems to me a sort of enchantment. The present becomes almost nothing—a mere vapour—and the past becomes so distinct that I recognise the steps of the departed as well as their voices. The most trivial incidents rise up before me wherever I go; and in every room of the house, and every walk of the garden or woods, I see again the old gestures, expressions of face, even accidents of dress, which no one could fancy could have lived in memory. I allude of course to my father and mother principally, but not to them only. Very old friends, most of them long since dead, walk with them; and the old jests are repeated, but with a strange mixture of pathos and mirth; and my brothers and sisters (there were two younger sisters who died each at the age of fourteen) seem to me once more as in the old days of childhood or opening youth.

'I suppose that many people must have had this experience. To me it seems nearly the most solemn and pathetic one that life brings. With all its melancholy the preponderating feeling it in-

cludes is one of sweetness. As life goes on, and goes by, a certain tenderness, almost remorseful tenderness, seems to attach to all human relations—and even to inanimate objects. We seem to be such mere shadows—such a *helplessness* seems to belong to us, and our parents, and all the fleeting generations that glided on and off the stage of life, in days earlier than those we remember, that it appears an inconceivable cruelty that anyone should "agitate the light flame of our hours" by unkindness to a neighbour, or even by want of sympathy. It seems as if everyone must have been intended to be in some sense a "helpmate" to everyone else, and that to hurt instead of helping any of those who belong to so feeble a race—a race whose joys are so fleeting, and whose trials are so many, irrespective of those which come from a fellow-being, implies that a long madness is preying upon human society.

I am writing in the old library, and the writing-desk on which my father wrote all his poems and letters is the one my paper rests on. Among the Phantoms which have been visiting me is a Phantom visit from Adare. One of the party is a young Philosopher of whom we had already heard as a Scientific Wonder: and now from his discourse we find that he is as much devoted to Poetry and Metaphysics as to Mathematics. Then there comes the ramble in the woods, the departure, and my father's enthusiastic comments on the young Philosopher, and especially the praise bestowed on the "transparent candour" which made all his thoughts seem visible things.

'Even those whom I never saw, but who belong to this magical Past, share in the strange spectral Resurrection. There is poor Francis Edgeworth. We used to talk of him and his poetry; and as I walked about our lawn yesterday there came before me as freshly as ever his poem with the lines

> "It cannot soothe me in my loneness;
> It cannot mitigate my oneness."

'Such a strange beauty belongs to the Past that it seems to me as if half the value of the Present is derived from the knowledge that it will one day be the Past. It often happens that the moment a friend has left the room (especially at night when they separate till morning) he becomes invested with a new character; and a softness falls on the image (that lately wore all the roughness of a jarring Present) from the distant Shadowland. Of all

the feelings of this sort, the strangest is that which comes to us from the recollection of our parents at an age when they were, as we then thought, getting oldish, but which, looking back on it, we see to have been Youth—the Youth we have ourselves lost in turn. To look on them thus with filial reverence, and yet with that *protective* sadness with which we regard the young and inexperienced who are unconsciously losing what they must so often lament, is a mixture of feelings which admits of no name. It makes one realize the facts that even the Parents of our Parents, who seemed to us like Patriarchs, had their brief day too, and were but poor gentle wrecks stranded in the sunshine, when we knew them. The feeling thus inspired seems to me to help us to conceive that relenting tenderness with which the great Father must look on all His creatures, remembering that "they are but clay," and that with all their high hopes they have been so mysteriously "made subject to vanity." However, the next World is the world of realities; and here the shadows rehearse at least the great Future drama. For almost all of us there must surely be some intermediate place where we are made capable of looking unblinded on the wonders of the Future. To me there always seemed something so unphilosophical as to be almost incredible, in the *assumption* that death (which is not even a sacrament) should so act as a charm or talisman, as at once to render the mass of even the good, who have hitherto been occupied mainly with the Shadowland, capable of sustaining the uncreated glories of the Beatific Vision. It is not a question of *Guilt* or of *Merit*, but of the time necessary for the development of moral habits and spiritual capacities, as opposed to the notion of magical transformations.

'I quite forgot that I had altered the hymn on Good Friday, till your letter made me refer to it. It is, I think, on the whole improved.'

In December of the year 1880, Aubrey De Vere published in the *Irish Monthly Magazine* two sonnets 'In memory of Sir William Rowan Hamilton,' which, as he says in a letter to me, 'show at least that Hamilton was one of those, one's friendship for whom will stand the wear and tear of some fifty years.'

Of these sonnets I reserve one for a later page; the subject of that which follows suggests its insertion here.

'IN MEMORY OF SIR WILLIAM ROWAN HAMILTON.

'AFTER READING AGAIN HIS LETTERS.

'At times I see that ample forehead lit,
Bright as the day-spring round the mounted lark;
At times I see thee stand in musing fit;
At times in woodlands of that twilight park,
Deciphering well-loved names on beechen bark:*
Where Rotha's moonlight ripples past thee flit,
I see thee kiss a grave—then by it sit—†
Her grave that left the land's chief Poet dark.
This day I read thy letters. Word and scene
Recur with strangely mingled joy and ruth;
Thy soul translucent, yet thine insight keen,
Thy heart's deep yearnings and perpetual youth
Thy courtesy, thy reverence, and thy truth—
All that thou wert, and all thou might'st have been!

'*February* 20, 1880. 'A. DE V.'

Memoranda respecting the Meeting of D night, July 6, 1863, of the Royal Irish Academy. [Manuscript Book L. 1863, p. 13.]

'On D the 6th I went in the evening to attend the Special General Meeting of the Royal Irish Academy. That meeting had been convened by the President and Council, to take into consideration some proposed Resolutions on the subject of a Report recently published, of certain Royal Commissioners, who had, as the Council conceived, and as the Academy voted, suggested our being placed to some extent under the control of the Council of the Royal Dublin Society.

'The whole affair had been managed with a most suspicious secrecy on the part of the Commissioners; and an impression exists that there has been a *job* at bottom. A good deal of indignation was expressed by several speakers, of whom I was one, coming next after the Treasurer, Dr. Carson, who followed the President, Dean Graves.

'The President stated, that as soon as he received (about ten

* Vol. i., p. 515. † *Supra*, p. 28.

days or a fortnight ago) information of what had been recommended by the Commissioners, he did not lose an hour in writing to the Lord Lieutenant, to protest on the part of the Royal Irish Academy, of which he was the head, against any such recommendation being adopted. And in my speech I expressed a hope that the Academy would vote their unanimous thanks, before the Meeting separated, to Dean Graves for his prompt, spirited, and useful action in the matter. Accordingly, when the Resolutions sent down by the Council had all been voted, with only some verbal changes in the two last of them, it was proposed and carried by acclamation, that the special thanks of the Academy should be given to the President; further action being remitted to the Council.

'I was led to state that I entertained only respectful and affectionate feelings towards the Royal Dublin Society; and that I was not insensible to the honour reflected on Dublin, and even on Ireland, by a Society so ancient and so useful.

'I valued much the compliment which had been paid me, many years ago, by my election as honorary member of that body; and had at least on one important occasion availed myself of my privilege to *speak*, although I could not vote, in the Society.

'And to show more completely how far I was from feeling any disrespect or unkindness in that quarter, I ventured to relate a circumstance which had occurred many years ago, and which I was not in the habit of talking of, although at liberty to do so, because I had taken no oath of secrecy on behalf of myself and fellows at the time.

'It had happened, then, about twenty-two years ago, that I was a member of a Commission which met at Mac Cullagh's rooms in College, and to which the consideration of the affairs and constitution of the Royal Dublin Society was submitted. And it then appeared to me that I was *at first* the *only* member of that Commission who did not aim at the *abolition* of the Society, or at least at a *reconstruction* so complete as practically to amount to abolition.

'But I had been firm in my opposition to any such measure or recommendation, and had given it to be distinctly understood that I would withdraw from the Commission if such were to be made the basis of its Report. I prevailed; and it had ever since been my feeling, although seldom if ever expressed, that *I had saved the Royal Dublin Society.*

'That Society was *older* than our own, which had not existed quite eighty years; it was also richer; and on each of these two grounds, but especially on the first of them, a certain *precedence* of *ceremony* might cheerfully be conceded to it by the Royal Irish Academy. It was not merely that the President of the Royal Dublin Society happened also to be the representative of the Sovereign: were he only a private member, elected to that Chair, "You, sir," I said—addressing myself to Dean Graves—"would not be humiliated by allowing him *precedence* on any occasion of *state* or *ceremonial*, on the ground of that relative *antiquity* of the Institution which he was thus supposed to represent. We want no Ulster King, no Garter King-of-Arms, to settle *that* point of precedence. But if it be proposed to concede any other sort of precedence, or to allow any kind or degree of *control* to be exercised by the Council of the Royal Dublin Society over the Royal Irish Academy or *its* Council, then, sir, I must reject such a claim with an *indignation* which I am sure that I do but share with every member present."

'I do not now (3 July 7, '63) assert that those were my *exact words* last night, but I remember that I spoke in that *tone*, and made remarks to that *effect*. My speech was not a very short one; it was animated, and (I think) well received; but it was entirely extemporaneous. At one stage, having previously mentioned Mac Cullagh in another connexion, I again introduced his name, and said that I could conceive how indignant *he* would have been, if he had lived to see a proposal thus made to subject to the *control* of *any other Society* the Academy to which he had devoted so large a share of his gigantic intellect in science, and on which he had concentred so much of his deep love of country.

'It was, I think, *then* that I made a transition to *myself;* to my own history and feelings. I said that I could remember the time when, being but a boy, and not yet aspiring to the honour of being a Member of the Royal Irish Academy, I at least looked forward to that of being permitted to write in its pages; and felt, what has been my feeling ever since, that any contribution so received would be an offering laid upon the altar of my country. I could remember the pride with which I found myself elected a member of the Academy in 1827, and of the Council in 1828; to which was added the still higher honour, in 1837, of being called upon to preside

over the body for a considerable course of years, with the unusual compliment at the close of such Presidentship of receiving a vote of thanks for consenting to remain, for some years longer, a member of the Council. And if I had not been included in the last list of *such* members, I felt that no slight was designed thereby, but that it was known that I was engaged upon a scientific work, which would prevent my frequent attendance, and of the subject of which, although I might occasionally give some account to the Royal Irish Academy, I should certainly never think of laying any such account before the Royal Dublin Society; but if I could suppose that the Academy would *submit* to any such *control* as was proposed, I must ask to be forgiven when I said that I should *then* scarcely feel *membership* to be an *honour*.

'I observed that we had no reason to apprehend on the part of the Government any attempt at coercion. The Academy and the Government for the time being had always been in friendly relations. I had myself the honour, on four different occasions, to receive, as President, the Viceroy of the time; and "you, sir," I said to Graves, "have had some similar opportunities. We have no reason to apprehend hostility from any such quarter. *But*, if the worst comes to the worst, *we must not* sell our Charter."

'I then sat down, but remember that I had *begun* by stating that I had inquired whether there were any *programme*, or *plan* previously arranged, according to which the order of the speeches was to be regulated; for that, if so, I would have contented myself with a few words, before the close of the meeting. Considering the whole occasion as an *exciting* one, I had been anxious to say as *few* words as possible, and had in fact kept myself in a *cold bath of mathematics* all the morning, that I might come to the subject as calmly as I could. It was only a very few days ago that I had heard anything at all respecting it; and I might mention that last Saturday, when I happened to have some conversation respecting the proposed resolutions with a distinguished intellectual man [it was Dr. Lloyd, after the Visitation], who happened to lunch with me, he said to me that they were very good, "but, don't you think, Hamilton, that they are a little *warm*?" "Warm?" I replied, "*of course* they are warm; would you have had the Council send us down a *cold* set of resolutions on such a question?"'

CHAPTER XLIX.

EXCURSION IN WICKLOW. DR. INGLEBY AND METAPHYSICS. HAMILTON'S POETRY. 'DECLARATION OF MEN OF SCIENCE.'

(1864.)

THIS year was with Hamilton one of enormous diligence in mathematical investigation and correspondence, his manuscript books and letters several times recording work extending in the day to beyond twelve consecutive hours.* The only intermission of any length which he allowed himself was occupied by a few days' excursion in July, taken in company with his daughter, through part of his favourite recreation ground, the county of Wicklow. His daughter notes that in this excursion, while evident tokens appeared of the decline of his muscular strength, his bright companionableness (shown, for example, by discussions with an intelligent Protestant carman on the parable of the Unjust Steward) and his youthful spirit of enjoyment were quite unimpaired. His own record of this excursion in a letter to De Morgan, dated New Hotel, Glendalough, July 25, 1864, makes mention of an interesting fact of which I do not remember elsewhere to have met any trace, namely, his accompanying Wordsworth to Glendalough.

'. . . I had visited these lakes of Glendalough in company with the poet Wordsworth so long ago as 1829, on which occasion I was young enough to climb from the lake-shore into St. Kevin's Bed—an ascent not quite without danger, at least

* One letter to his son Archibald says (June 6): 'This morning I was up at four A.M. . . . I have been busy ever since.' Another (September 12): 'I have been up since long before daylight this morning, but am not yet fatigued.'

to people who have not learned to swim. But at the time I was familiar with both swimming and climbing. I have been in this neighbourhood many times since, but do not remember that I *ever* enjoyed more thoroughly a visit to it, or to various other parts of the Co. Wicklow, than on this last occasion, when I have been touring with my *daughter only*: Lady Hamilton, though wonderfully recovered from a recent illness, not feeling herself quite strong enough for the exertion and fatigue—which last was not altogether to be despised by ourselves, especially as we chose to travel back to back on an outside car. . . .'

I select from his scientific correspondence a few passages which I think will be judged to be of interest either as throwing light upon his work and character, or as bearing his testimony to the qualities of his correspondents.

From A. S. HART, LL.D., S.F.T.C.D., *to* SIR W. R. HAMILTON.

'TRINITY COLLEGE, *January* 25, 1864.

'On Dr. Lloyd's return from Italy he told me of a conversation he had had with Plana about the quantity of heat received directly from the sun during the year at different points on the earth's surface. Not knowing where to find Plana's investigations, I attempted the calculation myself, but have got no results beyond those on the enclosed Paper, and they are so unlike what I had expected that my faith in them is weak, although I cannot detect any error. Did you ever happen to turn your attention to the matter, or have you any Paper of Plana's on the subject?'

From SIR W. R. HAMILTON *to* A. S. HART, LL.D., S.F.T.C.D.

'OBSERVATORY, *January* 26, 1864.

'You have so remarkable a talent for simplifying things which appear difficult in Mathematics, that I by no means despair of following your analysis throughout in your last note to me, although I am rather rusty in elliptic integrals, with which, however, I long ago connected (without publishing the connexion) the quaternions, as I had the pleasure of proving to my affectionate host of the time, Dean Peacock, at Ely, in 1845, when he asked

me whether I had yet thought of establishing such a connexion; and I produced on the spot, from my modest luggage, a manuscript book containing a sketch of such an application.'

Subsequently, 'I have just an impression that Plana claims for Poisson the merit of having converted a *discontinuous function* into one of *continuous form*, and asserts that Fourier (which, if true, is wonderful) had *failed*, or at least omitted, to do this.'

His next letter to Dr. Hart puts forward a notion illustrative of his characteristic power of generalisation, and his inability to be satisfied with superficial distinctions, however generally received. Its importance induces me to print it entire.

From the SAME *to the* SAME.

'OBSERVATORY, *April* 13, 1864.

'I write just a line to warn you that I have indulged myself by taking a large ruled sheet to write to you upon, which may have a successor, but which you will be most welcome to throw aside, whenever you may receive it, although any remarks of yours on the subject will be valued. Quaternions do not of *necessity* enter into the exposition of any part of the views on which I should thus like to consult you; but it is natural that they should have modified the formation of those views, so far as they are mathematical.

'The main point is, that the *whole* of what we call STATICS is really a part of *Dynamics*, except in books; there being no such thing as *Rest* in the world, unless (by a bare possibility) in the centre of gravity of the universe.

'All this has a bearing on Foucault's *Pendulum* experiment, of which I think that I have lately given a very simple and satisfactory (but as yet unprinted) explanation by the Quaternion Analysis. But I carry it severely on to what *seems* an incomparably simpler question, namely, to what I presume to call for the moment *The Plumb Line Experiment;* tried every day by Mr. Thompson here, or by myself, when we close the shutters to let the plumb line hang undisturbed by currents of air, and see that the wire bisects a dot, and *continues* to do so.

'When that interesting event comes to pass, he says, and so do

I, for ordinary purposes, that *the plumb line is at rest.* But *we* do not need to be told—nor does he—that in point of fact, *annual motion* being abstracted from, *the weight is in motion*, describing (in this latitude) something like 900 *feet eastward per second.* Instead, then, of saying "What keeps the plumb line *at rest?*" we ought, for greater accuracy, to inquire "What *makes* the weight describe its *diurnal orbit* (the parallel of latitude)? Of course, *you see*, the moment the question is stated, that the *vis acceleratrix* in this circular motion is the *resultant* of the earth's *attraction*, and of the *tension* of the string, at least if the mass of the suspended body be unity; but is *this* at all a *usual view*?

'If you give me any encouragement I shall perhaps pursue the subject in letters to you, for a short time, in the *least borish* way of writing, which may be the most openly and *ostentatiously borish*, by writing on *large ruled sheets.* But it is just as likely that, after indulging myself by expressing my views, I may quietly put the sheets in the fire.'

In the month of May, Dr. Hart, who had been furnished by Hamilton with proofs of the *Elements*, now reaching to page 696, adds to an acknowledgment the following:—'If I might venture a suggestion, would it not be advisable to close this volume with the geometrical applications and leave physics for a second volume?' In November his desire, on behalf of himself 'and the present generation' to see the great work given, even in part, to the public having become in the meantime, very naturally, rather impatient, Dr. Hart repeats his suggestion, urging in support some very good reasons. Hamilton's reply seems to have been only the sending to his friend some more proofs, which brought him the gratifying reply which I here give:—

From A. S. Hart, ll.d., s.f.t.c.d., *to* Sir W. R. Hamilton.

'*November* 23, 1864.

'I hasten to retract any objection I made to including Physical Applications in the *Elements*—at least so far as Statics. I had no idea that you could condense so much into three pages, and I should be very sorry indeed that such a full and concise theory of

the Statics of a Rigid Body should have been suppressed, especially as the two ideas of the centre of a general system of forces, and the tension of a system of equilibrating forces are new to me and perhaps to others also.

'If your dynamical applications are equally concise and pregnant, they will certainly form an admirable conclusion to the Work.'

Hamilton was now again becoming anxious about the cost of printing and publishing his book. He shrunk from applying to the Board of Trinity College for more pecuniary assistance, and, in prospect of negotiating with publishers, thought that attestation from some high quarter to the value of his researches might aid him in gaining favourable terms. With this view he wrote as follows to his old friend Dr. Romney Robinson:—

From SIR W. R. HAMILTON *to the* REV. T. R. ROBINSON, D.D.

'OBSERVATORY, *December* 5, 1864.

'I am, as I hope, approximating to the Moment of Projection—or, in plain English, drawing to the time of publication of my long and laborious work, entitled the *Elements of Quaternions*.

'It is only, however, within the last two or three days that I have opened negotiations on the subject with my old friend George Smith (Hodges, Smith, & Co.), and I have not seen him since I wrote last week, partly because I am hampered by a very heavy cold. But it is arranged that I call upon him, if possible, on Wednesday next (the day after to-morrow).

'The total expense of the work—which is not yet *quite* ready for publication, though very *nearly* so—will have amounted to about £400, whereof the College will have kindly borne £200, that is about one *half*. The remainder, even if there should be *no* remuneration, however small, to the *author*, may well deter a cautious publisher.

'Quite lately Dr. Hart—who, as Senior Fellow, Bursar, and Friend, has watched the entire progress of this last work of mine—has been pleased to write to me the kind note of which I enclose a copy made by my son William Edwin. . . . Dr. Hart and Professor Tait have been in fact the only persons to whom I have sent

copies of the printed sheets *regularly*. But I indulged myself lately by sending to you five half-sheets of my *Contents*, and also the last printed half-sheet of the *Elements* themselves. And it seems not impossible that even *those* may have served as some *materials* for forming an opinion on your part.

'If you can honestly say—and I know that you will not say it otherwise—that your general *impression* is favourable to the work, and especially that you think it treats old and celebrated subjects of science from a *new point of view*, I shall be glad: but you see that to render your reply *useful*, it should be prompt.

'For my own part, on reading over lately those Essays of mine in the *Philosophical Transactions*, which won for me, thirty years ago, the applauses of the whole scientific world—so much so that I have been quite tired, though of course flattered, by meeting so many references since to "les équations Hamilton," &c.—my feeling was that I was *not ashamed* of those old Memoirs; but that they belong to a *past age* of Analysis, so completely do Quaternions appear to me to furnish an ἀνακαίνωσις of mathematical, and, through it, of physical science.'

'*December* 6.—I find that I omitted to enclose, in my packet of yester-evening, the copy of Dr. Hart's note, which I now send. Of course Dr. Hart could have no motive to flatter me, and the warmth of his expressions is to be traced to a sort of penitence for his having very strongly, though with all kindness, urged me previously to let the *Elements* appear, as a *purely mathematical* work, all *physics* being reserved for some future occasion. But I am too old to trust to the future.

'We all enjoyed much a recent visit from Francis Edgeworth —who permits me in conversation to call him "Frank"—for to me there can be no second "Francis."

'With kind regards to Mrs. Robinson and to Mrs. Stokes, when you can forward them, I am,' &c.

Dr. Robinson's reply was promptly despatched:—

From Rev. T. Romney Robinson, D.D., *to* Sir W. R. Hamilton.

'Observatory, Armagh, *December* 7, 1864.

'I am very glad to find that by the approaching publication of the *Elements* we shall be put in possession of your latest develop-

ment of this magnificent branch of Analysis. As far as I can judge from the proofs of the *Contents*, which you kindly sent, it will be of great use in making more generally accessible the wonders and the wealth of the field which you have so happily opened. The Statical and Dynamical applications of it in § 416 and § 417 are almost startling from their brevity, yet power and extent.

'I hope nothing may occur to delay the appearance of *the book*, and in this I am sure all my mathematical friends will heartily concur.

'I fear the Royal Society is not allowed by its laws to give pecuniary assistance to publications with which it is not officially connected; otherwise I think an application to it would be successful.

'I congratulate you on this conclusion of your great work: not as a final resting-place, however, for the field is infinite.

'Ever yours.'

From SIR W. R. HAMILTON *to* REV. T. ROMNEY ROBINSON, D.D.

[FROM A DRAFT.]

'OBSERVATORY, *December* 19, 1864.

'I regret that I have not yet thanked you for your kind letter of ten days ago. It was alike honourable to you and to myself. I have not yet attempted to make any *use* of it; but have allowed two recent guests of mine to read it. . . .

'My last visitor has been the Rev. Robert Perceval Graves, brother of Dean Graves, and a very old and dear friend of mine, who slept here last night and left me to-day. The room which he occupied, and which other guests have occupied lately, shall be very much at the service of Mrs. Robinson and you, if you should pay me the compliment of spending a few days and nights here, when the weather becomes finer. I am, &c.

'P.S.—When I know more, I shall mention more, of the prospects of my book. Meanwhile, like Milton, I bate no jot of heart or hope—except as regards money, which is the least important item in the matter.'

In June of this year Hamilton gave himself a little diversion in the way of arithmetical calculation, connected with astronomy

and chronology, as recorded in the following letter to his second son:—

From Sir W. R. Hamilton *to* Rev. Archibald H. Hamilton.

'Observatory, *June 27th*, 1864.

'I have recently felt a curiosity to calculate back to the date of the Hegira (or Hejira), from the statement in the first page of Thom's Almanac, that the Mahomedan year 1281 begins on our 6th of June, 1864, and from my remembering that a Mahomedan year consists of twelve lunar months, whereof the mean value may be safely taken as 29·5306 mean solar days. (Laplace, indeed, assigns 29·530588716 days as the *present* value, perhaps for the commencement of this century, of the *mean lunation*, or *mean synodic revolution* of the moon; but besides that this value is very nearly = 29·5306, the *month* is *slowly diminishing*, by the *secular acceleration* of the moon's motion.) Multiplying then 29·5306 by 12, I got, first, 354·3672 days as the *average* length of the Mahomedan year; and then multiplying this by 1280, I found, very nearly, 453590 days, as the interval from the Hegira (or from the commencement of the Mahomedan year 1) to the 6th of the present month. Dividing 453590 by 365·25 (which latter is the average length in days of the Julian year) I got *nearly* the quotient 1242; whence it might be at once inferred that the flight of Mahomet took place in the *year* 622 (A. D.). But to fix the *time* in that year, at least nearly, it seemed convenient to proceed as follows. 1240 Julian years = 310 × 1461 days, = 452910 the above calculated interval—680 days; and our 6th of June last was in *Russia* only the 25th of May; the Hegira then preceded the 25th of May, 624, by 731 days. Hence (if the foregoing *data* be admitted), the Hegira *followed* the 25th of May by 51 days: it fell then on the *15th of July in the year 622* A. D.

'Such, at least, is the final result of the foregoing calculation; but I placed no great reliance on its *minute* accuracy till I *subsequently* found Herschel's *Astronomy* in one of the scientific shelves in the Library, and had the satisfaction of seeing, in page 635 of that work, the precise date, July 15 A.D. 622, assigned as that of the *Hejira* (so spelt by Herschel).'

'What set me thinking of the date referred to was my

recently happening to take up Helen's copy of *The Adventures of the Caliph Haroun Alraschid, recounted by the Author of Mary Powell . . . London, 1855.* Who the interesting authoress is we forget, if we ever knew. There is a not unpleasant air of oriental learning, got up no doubt for the occasion, but not unfair, since some references are given in the margin. But the very first marginal note is as follows: "Of the Birth of Haroun the Just. Year of the Hegira 139, A.D. 761." Then come at intervals the dates:

A.D. 770.	A.D. 786.	A.D. 807.
Heg. 148.	Heg. 164.	Heg. 185.

'The process of manufacture, you see, is very simple. Miss *x y z* simply *subtracts* 622 from A.D., in order to get *her* "Heg."! I saw that this could not be right, whatever the A.D. date of the Flight may have been. *Her rule* would have made *this year* = Heg. 1242; whereas it answers to *part of* 1280, and *part of* 1281, of the Mahomedans. I could not at first find a single book of dates, and was glad to calculate for myself as above, and I am, dear Arch, your affectionate Father.'

That this stern detector of miscalculations in chronology was not disposed to be a harsh critic of authoresses is shown by the following extract from a letter written not long before the above:—

'OBSERVATORY, *April* 29, 1864.

'DEAR MISS ALCOCK,—As you and I are *both* friends of my daughter, I may perhaps be permitted to consider myself as entitled to offer my thanks to you for the very great pleasure (I may well add instruction) which I have received from the perusal of your beautiful and valuable little work on Alfred the Great and Good. It may not be known to you, that Mr. Wordsworth, the Poet, . . . claimed to be a lineal descendant of Alfred. . . .'

For a considerable time in the course of this year Hamilton's mind turned to metaphysics; and Plato, Aristotle, Descartes, Berkeley, Kant, Coleridge, Sir W. Hamilton of Edinburgh, and Dr. C. M. Ingleby furnished him with matter of thought. This

digression from his ordinary occupation, or rather I should say these occasional diversions, were caused by his having received from his new friend, the last-named in the above list, the First Part, recently published by him, of an *Introduction to Metaphysic.* I prefix to the extracts from letters of this year to Dr. Ingleby one written in 1861 on the morning of the day of which the evening was spent by the two friends at the Observatory in their first conversation on Philosophy.* It fitly introduces the rest of the set by which the reader is prepared for still further treatment of the same subjects in letters to other correspondents. Whatever may be the inherent value of the contents of these letters—and on this point I do not pretend to be qualified to pronounce an opinion—they are characteristic of the writer, and deal with interesting topics.

From Sir W. R. Hamilton *to* C. M. Ingleby, D.C.L.

'Observatory, *August* 21, 1861.

'From your extremely obliging note received on Monday morning, I collected that unless I wrote to the contrary, as being otherwise engaged, &c., you would favour me with a visit this evening. I pray you to accept that impression as an excuse for my not having written to you since, though I might mention that I was working, on Monday, for example, for *more than twelve* consecutive hours, on things connected with my forthcoming volume, the *Elements of Quaternions.* I happened to notice this morning, while relaxing myself—for change of labour *is*, to a studious man, a relaxation—with some perusal of Plato, in a vast folio of M.DCII., that 9 Quaternities had been picked out of his writings by old commentators, *e. g.*

SECUNDA.

Cratylus: vel de recta nominum ratione.
Theætetus: vel de scientia.
Sophista: vel de ente.
Civilis: vel de regno.

* *Supra*, p. 135, where the Christian name of Dr. Ingleby is printed 'Charles' instead of 'Clements.'

'I rarely look at the Latin column; but in this case there was no help for it, because there was no Greek to match.

'If anything should unexpectedly prevent your coming here this evening, I beg you to believe that I have attained *so far*, in the study of Plato, as to be most unfeignedly aware of my *immense* inferiority, in *Philosophy*, to *the* Sir William Hamilton.'

From the SAME *to the* SAME.

'OBSERVATORY, *May* 26, 1864.

'. . . Your *Part* I. [of Dr. Ingleby's *Introduction to Metaphysic*], above acknowledged as arriving, has interested me *extremely*. . . . It is long since I have *read* metaphysics, as in any sense a student; but the Paper quite revived my old interest in such subjects. I felt that I examined better on the Tuesday in consequence of having so spent the evening previous; and on the following Wednesday, when I went to dine with a co-examiner, Professor Jellett, at whose house (seven miles at least from this place, according to my estimation) I met, among other pleasant people, my old friend Dr. Lee, now Archdeacon of Dublin, I took *Part* I. with me to read in the cab, and to think on; and on returning to this Observatory, at about half-past twelve, sat up for at least two hours, to read and think on it again.

'I had retained a recollection, confirmed by your letters, of your being a very agreeable and highly cultivated gentleman; but was scarcely prepared for finding you turn out so much a *power* in the world as your new "Parts" seem likely to rank you. If you had published anything previously, I should be very glad to see it, and very willing to purchase it: for I like your *style*. "Le style, c'est l'homme," say some: it gives at least an indication. At the same time, as a sermon which everybody praises does no good, you would not be satisfied, I suppose, if I had no objections, or at least difficulties, to offer for your consideration.

'Thus in § 6, sentence the second, I should like to know whether you deliberately teach, respecting the Ego, and the Non-Ego, "that whatever is not one is the other." If you do so teach, I still do not understand, as a logician, your saying immediately afterwards, "*i.e.* that they exclude each other." Surely A and B may exclude each other, without the non-existence of some *third*

thing, or thought, C. At this moment, and with a deep sense of my want of recent preparation for entering on any such high investigation, I should be disposed rather to form this little table or scheme (not strictly in the Kantian sense, perhaps, of this last word):

{ Der Ich. Das Nicht-ich.
Die Gemeinschaft.

or in Latin:

Ego. Non-Ego.
Medium (Communitas).

'On turning to your § 64, page 61, I see that you use the *words*, "a medium between Ego and Non-Ego"; but, as it seems, rather to reject a view in which "Organism" might be taken to be such. What you say in the same paragraph, but in the following page (62), comes much nearer to what I mean. You say: "How Ego can be disturbed by Non-Ego is, indeed, an inscrutable problem. The fact must be taken as the subject of a primitive faith." But to complete for the present purpose the outline of my view of the above hinted-at *Medium transcendentale*, between the Ego and the Non-Ego, I would refer to your § 68, in which you say that "Correspondence and mutual dependence without likeness is the watchword of this system." And what is still more important for the immediate purpose, you say, in the same page, "every change which we voluntarily effect in the latter world" (that of the senses) "has its concomitant and corresponding change in the former." You admit, then, that the Ego can *disturb* the Non-Ego, although in a manner to us entirely *unknown*. This I call a *part* of MEDIUM (perhaps rather a *phase* of it. I am reminded of Coleridge's "Punctum Indifferens," sive "amphotericum," which as viewed from A is B, and as viewed from B is A); the *other part*, which we *know*, because *we construct it*, according to your very able and important development of Kant's Æsthetic, if rightly understood by me, is what we call *Perception*. In the long drive to Jellett's dinner on Wednesday last I had the greatest comfort in saying to myself, *That tree*, that spire, is truly *there*, as I see it—because I have constructed it, though not without suggestion from the Non-Ego, as an *object* so located. I had no difficulty in turning to the passage from which your motto is taken, and which very early struck me in the *Kritik*.

'Hoping that you will not regard me as having been too free in my remarks, I am,' &c.

'OBSERVATORY, *June* 14, 1864.

'I have no letter of yours, nor of my own, beside me at this moment; and there are at least *two* unsent letters of mine, to which you are entitled. When I have *written* a letter I am apt to think my duty to my correspondent performed; the additional circumstances of folding it up in an envelope, directing, stamping, and in some cases copying, or getting copied, the contents, appearing to fade away into a remote perspective. . . .

'You mentioned, I think, a commentator who conceived, according to you, that Aristotle had written in Latin. I rarely read *new* novels (though I read fast), but am never tired of Walter Scott. Well, I yesterday lit, in his *Woodstock*, on the passage where the *learned* Dr. Rochecliffe remarks to Joceline: "Where, says the Septuagint, Percussum Egyptium abscondit sabulo." This is only *one* place, out of several, which convince me that Scott actually *believed* the LXX. to have been written in Latin. Did he coin the quotation? I have not a Vulgate at hand.'*

'*June* 4, 1864. . . . You were pleased to express, in a recent note, a fear, or at least an expectation, that some people will blame, or "superciliously smile at" you, for the "constant intrusion of physical and physiological considerations into Metaphysic." I shall not be one of those people. On the contrary, I conceive that it is most strictly proper and philosophical, in connexion with *Theories of Perception*, to try *all possible experiments*: and I may shortly indicate a certain "corpus," *videlicet* my own, on which some such might perhaps usefully be made. (Lest I should not have time to return on this, let me just say that I allude to my habitually *seeing double* since I was very young. I attribute this chiefly to my having had a telescope of my own—a very good sliding Dollond—when I was only about eight years old. . . .)

'You have most justly insisted on the *immense* superiority, which in fact I think *cannot* be exaggerated, of the *Sense of Sight* above the sense or senses of *Touch*. To hear some people talk, even people of highest note, or rather to read their writings, one

* It is in the Vulgate, Exod. ii. 12.

would be apt to suppose that a man *blind from birth* should actually have an *advantage* over a seeing one, in learning such a science as Geometry; that all would come *right* to *him*, almost from the *first*: or at the very least, that he would have *fewer illusions* to unlearn. Yet the existence of any such natural *superiority* of the born blind does not seem to be confirmed by experience. (Please to observe that I do not at all dispute the possibility of a man born blind attaining *any height* in mathematics: I only deny that he has any advantage over seeing people. Sanderson was certainly a remarkable man of science; but he had at least a *year* of vision.) For my part I have very long been of opinion that Touch—even *muscular touch*, *handling* as distinguished from mere *feeling*—has *no advantage whatever over sight*—even *over vision with a* SINGLE EYE—and this is a very important assertion for my purpose, in making known, or even *suggesting* to us the *Existence of Objects*, in TRIDIMENSIONAL SPACE.

'As to mere *skin-touch*, such as that with the palm of the hand, or other soft part of the body, the *information of objects* which it gives seems to be almost nothing, even at first contact: and in an extremely short time, such information, or even suggestion, appears to *fade away* completely.'

'*June* 4. . . . I doubt whether *skin-touch* would have led us to a *belief in objects* at all, any more than the sense of smell: less easily, I think, than that of *taste*, on account of the movements of the tongue; but I admit it to be impossible to say, *what* the slightest *spark* might or might not have done, when falling from what we call *without*, on the powder magazine of our Potential Intuition.

'But take our *Muscular Touch*—HANDLING. This sense gives, I think, *Two Dimensions of Space*, with ease; but *not with greater ease* (nor with *so much* to me, who am not blind) than the *sight of a* SINGLE eye, with its power of LOOKING, first at *one part*, then at *another part* of the *Picture*. *Books* tell me—*you* tell me—that in this *looking* I alter *the direction* of an *Optic Axis;* and I am far from disputing your authority. I submit to the teaching of OPTICAL ANATOMY. But MUSCULAR ANATOMY has other revelations to make, or doctrines to propound, which I am still less qualified to dispute; and of which I need only say that they introduce the element of *Body*—our own body—at least as much into what may seem the province of WILL, in the *Act* of *handling*, as in that of looking.

'To get *beyond either*, something *deeper*, higher, more *powerful*, is required, namely, the *Original Intuition of Space*, which waits to be *awakened by Sensation*.'

'*June* 15, 1864.—After laying the two preceding sheets aside for more than ten days, I am astounded, or at least amused, at my own audacity, in having written here and there, as if I had a right to an *opinion* of my *own* on such high points of philosophy. And *you* may be amused or surprised when I tell you that I have not, at present, a single book of Berkeley's or of Hamilton's in my library.

'As to Berkeley, it is of the less consequence, because I was early lent a good three-volume edition of his works by a noble friend, who was formerly a pupil of mine, and who, after about twenty-five years, reclaimed the loan not very long ago; so that I had leisure to become sufficiently impregnated with Berkeley's teaching, for one who has never aspired to be *himself* a *teacher* of Philosophy. In fact, when I was rather young, namely, in 1832, I allowed Coleridge (at Highgate) to see that I was at that time a regular Berkeleyan; and *he* was pleased to say—for our several interviews, of that year and the following, of which some were long, were not *all* monologues on his part—he allowed me to make a remark now and then, and actually modified his discourse to meet it: "Oh, sir, you will grow out of that!" In *some* respects that prophecy has been since fulfilled; but out of *love* and *reverence* for the great and good Bishop, I trust that I shall *never* grow.

Since, then, I have sat, although long ago, at the feet of Berkeley, what more disqualifies me than the temporary absence of *his* Works from my library, to be a worthy correspondent of *yours*, is the extreme slightness of my acquaintance with the writings of the great Sir William Hamilton—in whom you seem to have formerly recognised a master. (I have, however, read, several times over, the "Bampton Lectures" (for 1858) by Dr. Mansel, one of the most able and affectionate pupils of Hamilton, with its Preface, and most of its Notes [3rd Edition]...) A young friend lent me, in 1850, "Hamilton's Reid": this is probably not the exact *title* of the book, which he left with me for at least a year, perhaps for two. I was at that time very busy in carrying through the press my *Lectures* on Quaternions: as the *Table of Contents* of the *Elements*—a quite new work on the same

general subject—is occupying me just now. But I have a lively recollection of running over, as I might have done a clever and spicy *Article* in some Review, an APPENDIX—such, I think, was its title—in which Sir William Hamilton had (to me at least, and at the time) the *air* of vindicating what he was pleased to call the Doctrine (or something equivalent) of COMMON SENSE, by a tremendous artillery of learning and quotations . . . On the whole, I very much *enjoyed* the Appendix; but rose from it with the feeling that *no conviction* had been imparted or disturbed.*

'I totally *forget* if I ever took the trouble to understand what "cosmothetic realism" and "cosmothetic idealism" may have meant in Sir William Hamilton's Vocabulary. Now your pamphlet has set me thinking, with some curiosity, on such things again; but it has naturally produced an impression—*not painful*, because I am otherwise well employed, and there *must* be a division of labour—that I do not know the A B C of your present contest with your former master.

'. . . But, talking of *instinct*, it appears to me that no theory of *Human Perception* can be completely satisfactory which ignores *Animal Perception*. In Campbell's Specimens of the British Poets I have just noticed the following lines of an obscure writer (William Whitehead):—

> "The kitten, too, was comical,
> She played so oddly with her tail,
> Or in the glass was pleased to find
> Another cat, and peeped behind."

Whether this be true of a *young kitten*, I doubt: the lines perhaps do too much honour to its *intellect*, or *imagination*, or even *perceptive power*. But I have frequently seen a wise old *cat* look at herself in a mirror, and then, *at once*, go behind it. Have you ever troubled yourself to speculate upon this fact? And generally should you admit or deny that there is a good deal of *instinct* in our perception?

* On the 22nd of September, 1864, Hamilton writes to De Morgan: 'I continue to admire Hamilton himself [Sir W. H. of Edinburgh], as a writer, and should gladly be a *student* of his works, for many years to come, but am satisfied that I should never become a *pupil*. His whole *view* of what PHILOSOPHY is, or ought to be, differs essentially from mine. But it would not become me to enlarge at present on such a topic . . .'

'Human babies, I suppose, do not really *see*, until they begin to *take notice*, however good the *optical images* upon their retinas may be. But as to those *eye-images*, true as it may be that in strictness we do not see them, and even that, as being thus unperceived, they do not (Berkeleyanly) *exist*, *meta*physicians ought not to forget that *physicians* hold them to be, at least under ordinary circumstances, *conditions of vision*.

'I am pretty sure that Sir Isaac Newton has somewhere said that the great object to be aimed at, in the improvement of optical instruments, is to make *such* (retinal) pictures "bigger, brighter, and more distinct": or words to that effect.

'This has been so rambling a letter, if a letter it may be called, that I shall follow up here a remark on its 2nd page, by observing that although I *habitually see* a *double universe*, yet a marked improvement has taken place within the last few weeks, in my *power* of *seeing single*. This I attribute to my having lately, for the first time in my life, bought a stereoscope, and *used* it at leisure here: no doubt incited by your "Part." A friend, within a few minutes' walk of me, has long had a stereoscope apparatus; but *years* elapsed before I could catch the *effect* at all. With *each eye*, separately, I saw a good *relief;* but it was at Cheltenham, in 1856, that I first was able to see that TERTIUM QUID, which is the true result of the stereoscope: and certainly it greatly astonished me. The illusion was wonderful; but I cannot yet feel that it throws much light on my own ordinary process of vision. Whatever Berkeley may have said, I am quite sure that *I* SEE DISTANCE with *each eye separately;* and although the two focal lengths are not exactly equal, the universe seen with the one eye differs in no appreciable degree from that perceived with the other.

'I entirely repudiate the notion that it is *tangible distance* which I see: indeed I am not sure that there is *any such thing* as "tangible distance." What I *see* is, that some things are *nearer* than others; that there is a *gradation*, a *perspective*, which is almost exactly the same to one eye as to the other. It is true that, *if my attention is called to the question*, when I look (for example) from a window of an upper room on trees or shrubs in a lawn, I *can conceive* myself going to the room-door, thence to a lobby, thence down the stairs, thence out at a hall-door, and thence walking across the lawn, till I put my hand upon a tree or shrub.

But such is *not my habit;* nor do I admit that *any such reference* to a *possible touching* forms even an *element* in the *visual perception of distance*, which I insist that I HAVE (and with *each eye*), whether I *ought* to have it or not. As to *how far off* any given object of vision is, I admit that my estimate is very *vague:* or rather, I make habitually NO *such estimate* at all, as referred to feet, yards, or miles. I only *judge*, or rather SEE, as above said, that one object is *nearer than another;* and this often with an extreme variety, as in the case (for instance) of a *landscape.*

'The result of all this is, that *I* cannot attach *so much* importance as *you* do to *binocular* vision. If it had pleased Providence to bestow on me a *second pair* of eyes, I trust that I should have borne the infliction with patience, and have *learned* to *believe* in a single universe, while perhaps habitually *seeing four.*

'Having been from childhood a reader, and even to some extent a writer, if childish journals intermixed with scraps of Persian and Sanscrit, &c., are to be accounted writings, I certainly *see* a *page* of a *book*, or a sheet of a *letter*, as *single.* And *when I succeed* in *so seeing* a somewhat more distant object—as at this moment a cup of tea upon a table not very near me—I am conscious of *some new satisfaction*, and must admit that, in a way which I cannot thoroughly explain, I have more than before the *sensation* or *impression of reality.* This result may be partly physical, the object being then *more distinctly* seen; and perhaps partly *mental*, inasmuch as I had not previously *believed* in the *two cups* which I *saw.* I am almost inclined to admit that, as with the stereoscope, but in a much fainter degree, I see a *tertium quid.* But I am *not conscious*—and have tried the experiment very often—of any change of visible distance, such as I constantly, or at least normally, experience with the stereoscope, when I succeed with it, after some short double vision.

'Some people would not choose to be so candid . . . and I am not aware that any one could *guess* the fact [of my double vision], if not informed of it by myself. I had the honour, forty years ago—at which time I was a boy—of being introduced to Miss Edgeworth at Edgeworthstown; and she was pleased to indulge me with what I may venture to call an intimate acquaintance, or rather friendship, from that time to her death. She was an *eminently truthful* person, and it occurred to me to ask her, very early,

when we were by ourselves, whether I *squinted.* She looked steadily at my eyes for a minute or more, and then replied: "No, Mr. Hamilton, you *don't* squint." On the other hand, I once knew a very amiable lady, who squinted awfully; it was really painful to look at her face: and yet I have no reason to suppose that *she* saw *double.**

'The *case* may be more common than it is known to be; as "Daltonism" was found to be *not peculiar* to Dalton, who told me himself, that he could not distinguish by sight, or rather by *colour*, the fruit from the leaves of a cherry-tree. I am not aware of any deficiency of *that* sort in myself.

'To wind up the statement of my own *belief*, founded partly on experience, and partly on theory, regarding *my own* visual perceptions, I maintain, or at least believe, that *if* I had been *blind of one eye* from my *birth*, and had *also*, in some way, by malformation or paralysis, been deprived of the use of *both* hands from birth or earliest infancy, . . . I should *still*, by LOOKING, and LOCOMOTION, have learned to SEE, with the ONE *sound eye*, precisely the SAME VISIBLE UNIVERSE, *including gradation of distance* (I do not say *estimate* of distance, which, as already remarked, I do not habitually make), as that which, *with that eye*, I *now* behold. I remain,' &c.

'OBSERVATORY, *June* 21, 1864.

'I received your letter of the 20th to-day—but for the moment can only acknowledge it, by getting stitched into this sheet a number of others which had been lying by me [viz. the letters above printed from June 4th onward], and indeed are little worthy to be forwarded. Let me, however, say that I *think* I understand your *Law of Reciprocal Causation* (p. 61, &c.),† and have great

* [Dr. Ingleby annotates "I have. C. M. J."]

† '. . . The principle of *Reciprocal Causation* is too much an individual insight, of power and vitality, for the intrusion of Scepticism. In the table on page 64 I might, perhaps, have preferably employed *perceptus* instead of *perceptio*. But in the singular it is an *insolens verbum*. If a reader can securely catch the great distinction of *subjective* and *objective* experience (*i.e.* quâ percipient, and quâ speculator), he will not be long in perceiving, as I do, that the Understanding (Verstand) erects the *phantasmata æsthetica* into an objective *mundus sensibilis;* and resolves the Real *Mundus Transcendentalis* into a *Percept.* Obviously to do these two things is, in effect, to *convert the order of causation.* Who has ever seen, or, at least, said this before me?' [Extract from Dr. Ingleby's letter of June 20, 1864.]

hopes that it is both *new* and *true*. It certainly has given me several subjects of interesting meditation; but to render an opinion of mine of *any value whatever*, on *such* a question, I ought to have been more recently reading Kant and other metaphysical writers on the subject of PERCEPTION: and, above all, since you so often *refer* to him, and in a controversial way, I ought to have made myself acquainted with the views of *Sir William Hamilton*, otherwise than by an imperfect recollection, referred to in sheet 4 of the enclosed letter, of a partial perusal of his work on Reid, which was returned by me to a friend about twelve years ago. (I have lately been reading, with much interest, a good deal of Reid himself.) P.S.—Descartes, in Cap. vi., § xx. of his *Dioptrics*, says . . . "Ex quibus fit, ut ne quidem sensus noster communis, ideam distantiæ capere posse videatur, ultra centum aut ducentos pedes abductæ. Atque hoc patet ex eo quod Luna, et Sol, qui sunt e numero corporum remotissimorum quæ contueamur, . . . *pedales* ut plurimum, vel ad summum *bipedales* nobis videantur, licet ratio dictet, illos longe maximos et remotissimos esse." Berkeley, then, was *not* the *first* to think, with his Hylas, that the moon appeared to be "*about a foot in diameter.*" And *some such* estimate I believe to be *very common*, although I was too early *spoiled* by science to have a recollection of *my* ever thinking so. A *foot*, however, rather exceeds the *average* estimation, so far as I have noticed. Just now, I asked my daughter, "*How large* is the moon *in the sky?*" "About half a degree," she replied. "Come," said I, "you learned that from Astronomy; but answer as a girl of common sense." She knew, unluckily, that all depended on the supposed distance; still, by way of saying something, she named a *small saucer* as a comparison. I knew, long ago, a *very* short-sighted gentleman who, perhaps in consequence of the great irradiation in his eyes, compared the visible moon to a *small round table*, which might be of the extreme bipedal standard of Descartes. People in general, I fancy, don't trouble themselves to estimate the distance of the moon, *as seen* by them, but think that they *can* estimate its *visible size*, in feet or inches, or by some comparison of that sort.'

The following letter to Mr. Barlow, F.T.C.D., on Sir William Hamilton of Edinburgh, on 'Hamilton's Reid,' and on Descartes, is in natural connexion with the above Ingleby correspondence,

by which Hamilton's mind was led into the region of thought which in this letter he continues to explore :—

From SIR W. R. HAMILTON *to* REV. J. W. BARLOW, F.T.C.D.

'OBSERVATORY, *June* 30, 1864.

'. . . I have already been reading a good deal of the book you *re-lent* me yesterday (for this current month), known, I suppose, as "Hamilton's Reid." Reid's own works (at least his Essays) I had lately been reading with interest, in a well-bound two-volume octavo edition (Dublin, 1786), which was given as a College prize to the deceased uncle (Rev. James Hamilton of Trim) who educated me.

'Of Sir William Hamilton I am prepared to think even more highly than I already do. Were *you* not (I think you were) at, at least, *one* of the tea parties in Edinburgh in 1850, at Sir William Hamilton's house, to which we were invited together?

'I knew that he was an *anti*-mathematician; and had made up my mind that I would submit without reply to *any attack* on mathematics, from one who was so much *older* and more celebrated than myself, and was in infirm health besides. But Sir W. H. was so entirely the *gentleman*, as not to put my forbearance to any such test; and I still preserve and value the rather rare book, of somewhat modern Latin poetry, in which Leibnitz figures extensively, and which he was so good as to request *his* Lady Hamilton to fetch down for me from a shelf known to her.

'Well, I retain all my old respect for Sir William Hamilton, of Edinburgh, *as* a gentleman, and also as one of the most *learned* metaphysicians of modern times, at least within these countries. As regards the *logical* controversy between him and De Morgan, about the *quantification of the predicate*, and all that, I do not presume to have even an opinion, at present: perhaps, two or three years hence, I may be led to form one. (At Oxford, in 1847, I remembered enough of the, then, recent controversy, to set Mr. Hallam (of the Middle Ages, &c.) *thinking* from one breakfast to another. At the second breakfast Mr. Hallam admitted that the point was made out. By the *point*, I mean *De Morgan's view*, as stated by myself.)*

* See vol. ii., p. 495.

'But I conceive my old mathematical crony and correspondent, Professor De Morgan, to have *proved*—in the *Athenæum*, a few years ago—that Sir William Hamilton had *never* attained to a correct understanding of the *First Proposition* of the *First Book* of Euclid; although he used to lecture thereon, for the purpose (I believe) among others of illustrating his thesis (note to Reid, p. 709), that "mathematics are, as is universally confessed, *the easiest of all sciences.*" The italics are his own). This seems to me *unpardonable, in him.* I am most willing to *learn from*, but how *can* I *trust* a teacher, who ventured to *pronounce* on the intellectual tendencies and position of a science, in which he was *ignorant of his own ignorance* of the first rudiments? Still I am very glad to have his book.'

'*July* 4, 1864.—Since my letter of Thursday last has not yet been posted, I wish to write a sort of postcript to it. . . .

'I have an Elzevir edition (Amsterdam, 1677) of "Renati Des Cartes Principia Philosophiæ." It is said in the title-page to be: "Ultima Editio cum optima collata, diligenter recognita, et mendis expurgata." It contains several distinct treatises, bound up together, *e.g.* "Dissertatio de Methodo," "Dioptrice," "Meteora," &c. But the weightiest is the *Principia;* a title and (for the age) a success which *perhaps*, nay, I think probably, stimulated Newton to produce his own great work, although *it* was *Principia Mathematica.*

'As early as page 2, Des Cartes introduces his celebrated "Cogito" argument, under the form: "Ac proinde hæc cognitio, *ego cogito, ergo sum*, est omnium prima et certissima, quæ cuilibet ordine philosophanti occurrat." Observe the *ego:* it is repeated in page 3, where he does not deny . . . "non ideò negavi, quin ante ipsam scire oporteat quid sit cogitatio, quid existentia, quid certitudo," &c., but maintains that philosophers have rendered things obscure, which were in themselves most simple and well known, through seeking to *explain* them by logical definitions.

'Now this "ego," which Des Cartes thus repeatedly couples with "cogito," has very commonly been *omitted* in quotations; and I have even seen *arguments* founded on such omission, which sought to prove that the author of the sentence had *designedly* kept the pronoun out of view, and was thereby *sophistical.* His own remarks upon the whole subject are at least very *clear.*

'My edition of Des Cartes' philosophy, as already mentioned, is of 1677; but it was *not the first*, and Spinoza had previously written a sort of *mathematical commentary* on the first two Books of *that* "Principia." Accordingly, I have an octavo volume of 700 pages, published *also* at Amsterdam, but in 1663, and therefore 14 years earlier, of which a part of the title-page is as follows:—"Renati Des Cartes Principiorum Philosophiæ Pars I. et II., more geometrico demonstratæ per Benedictum de Spinoza Amstelodamensem. Accesserunt ejusdem Cogitata Metaphysica." . . . The volume contains also a "Tractatus Theologico-Politicus" . . . (Hamburgh, 1670), from which Colenso may have taken hints: besides Epistles, &c. Spinoza, in *his* page 4, uses twice, *without pronouns*, the formula: *dubito, cogito, ergo sum.*

'. . . There seems to be an incompleteness in the copy of the Notes on Reid which you kindly lent me. It breaks off abruptly at page 914, in the middle of Note D * * *; after which page come a couple of *specimens* of works preparing for publication. Have you a second volume? From the "Advertisement" I suspect that the volume, as published, may have abruptly ended; but in the body of the work I have observed references to Notes G and R, for example (compare the following passage), which are either non-existent, or at least non-apparent.

'In particular, and as connected with part of this letter, I observe that in p. 268 b, Reid says of Des Cartes:—"He used this argument, therefore, to prove his own existence, *Cogito, ergo sum.*" But in a footnote to the end of the following sentence (p. 226 a), Hamilton remarks:—"On the Cartesian doubt, see Note R. *H.*" Now, as above said, I can find *no note* later than D * * *; nor is even *that* note finished in your copy. But perhaps I shall find the cream of what Hamilton had designed to say in some of his *foot*-notes to Reid.

'I see that I must read Note C *before* Note A, on account of its technical terms.'*

In a very interesting letter addressed by Hamilton to Dr. Hart

* In Mr. Barlow's reply he says:—'"Hamilton's Reid" was, I believe, never completed, which is the more extraordinary, as Sir W. H. undertook and completed the editing of all Dugald Stewart's Works subsequently to the publication of the edition of Reid.'

on the 30th of June in this year, he thus refers to the metaphysical excursions, a record of which has just been placed before the reader:—

'. . . The fact is, that one of my early tastes was for metaphysics, and something has lately occurred to revive it. Another was for Eastern languages; and I chanced yesterday to light on the first sheet of a "Persian Grammar," written by myself forty years ago. These things, with others, may occasionally relax the bow: "non semper tendit . . ."; but "many tastes, one power"—and my only *power* is mathematics.'

This verdict, so deliberately pronounced by Hamilton, near the close of his life, upon his special faculty and function in relation to other constituent elements of his intellectual being, is in remarkable accord with the expression used by him at the commencement of his public career, when, in a letter to his sister (vol. i., p. 286), he spoke of his interest in Science as being his master-passion. Doubtless the word 'others' of the clause, 'These things with *others*,' in the passage above quoted, was intended to include Poetry: and the passage is therefore scarcely compatible with a declaration which has been attributed to him, 'I live by mathematics; but I am a poet.' I find it difficult to suppose this to be an accurate report of his words. He might very conceivably have said, 'What *I am* as a man is more shown by my poetry, which reveals the inner current of my life and my affections, than by my mathematical works; as also by my metaphysical, ethical, and religious opinions, which indicate my standpoint in Philosophy': this it would have been natural for him to say, for he felt it acutely,* but we may consider as certain that he would not, on

* A letter to myself gives expression to Hamilton's painful sense of the *inferiority* of mathematics, exemplified by the works of the highest mathematical genius, to corresponding works of poetical genius, through the absence from them of elements immediately affecting the spirit and life of man. The testimony borne in the first portion of the letter to Wordsworth's early appreciation of Tennyson had been anticipated by the elder poet's written words in

the ground of the occasional expression of such opinions in letters and lectures on Astronomy or of the occasional relief of his feelings afforded by his poetical compositions, have claimed a place in the rank of acknowledged Philosophers or Poets. He may, indeed, have been conscious of being potentially a Poet and a Philosopher, and that, I think, is all that he can ever have intended to express; but however largely he may have felt that he possessed the elements which constitute either the Poet or the Philosopher, it is satisfactory to read his clear recognition that his chief faculty was the power of dealing with mathematical truth, and to extend its boundaries his highest function. What he considered the main value of his poetry has already been recorded in the passages to be found in the second volume of this work, pp. 402, 612, 613; but I am able to add another more definitely acknowledging his sense of its defects, while referring to a favourable judgment on it of Francis Edgeworth, in which he evidently concurs.

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one of his letters to Hamilton (vol. i., p. 403), a fact which Hamilton seems to have forgotten, and, with regard to his notion of sending to Tennyson in return for pleasure and benefit derived from his poetry a copy of the *Lectures on Quaternions*, I remember discouraging it, on the ground that the poet could receive no reciprocal enjoyment from the book, which, in my opinion, was of too great importance to be used as a mere *token* of homage and obligation. It may be doubted whether my advice, which Hamilton acted on, was, taking all things into account, judicious; but its motive was what I have stated, and if it was mistaken, I have only to hope for the pardon of the Poet Laureate.

From SIR W. R. HAMILTON *to* R. P. GRAVES.

'OBSERVATORY, *July* 18, 1855.

'MY DEAR ROBERT GRAVES,—I have many social and affectionate debts to pay you, but it suddenly presses on my mind, that although (thank God) I am in excellent health just now, yet I *ought* to put beyond the chances of mortality my written testimony to the discriminating generosity of our great and departed friend, the Poet Wordsworth. He has been thought by some to have been unwilling to allow praise to other poets—and I never could enter, nor pretended to enter, into his criticism on Burns's "Scots wha hae wi' Wallace bled"—but *you* know that Wordsworth admired and *loved* Burns, and could appreciate him, and deeply and long regretted that he had not been acquainted with him. Mr. Wordsworth (we talk still of Mr. Pope) gave me a copy of his letter on the subject of Robert Burns, which expressed no stinted sympathy. Every one *now*

LETTER *to* PROFESSOR TAIT.

T. 1858, p. 139, '. . . It is not fair to ask my friends to be sincere about them [his sonnets]. That they have many faults I very well know; and take a sort of *pride* (or perhaps vanity) in knowing it: because that point of self-knowledge appears to prove that I have kept *too good company*, personally and intellectually, to be *satisfied* with any poetical production of my own. But Francis Edgeworth—a Cambridge man, although an Irish one—a younger brother of Miss Edgeworth, though by a different mother, who was during his life a great friend of mine, and for whose memory I retain respect and love, used indeed to criticise, *very sincerely*, some of my youthful verses; but wound up by saying, "After all, Hamilton, your poetry will not disgrace you."'

I will not here refrain from stating my own opinion, strengthened by that of friends specially competent to judge, that Hamilton's poems have, both in their diction and in their matter, qualities of enduring value; that, speaking generally, they are, and will always be felt to be, fresh, graceful, fervid expressions of states of feeling and thought, interesting in themselves and possessing a heightened interest from their being the heart's utterances of a man of gigantic mathematical powers and of strong and deep

admires Alfred Tennyson—there is no merit now in praising him: but you will bear with me as an old friend while I say that I have lately been reading, over and over—for I do not pretend to calculate how many times—the "*Princess*." I *may* indulge the hope, at moments, that as I now read, with profit and delight, the book of the great Grecian Mathematician, Apollonius of Perga, after an interval of two thousand years from its composition, so my own volume (of which I should be happy, if you thought you could manage it, to present Mr. Tennyson with a copy) may survive even several centuries—nay, that, as the earliest work in its own department, it may exist till books shall be no more. But it deeply presses on my reflection how much wiser a book is Tennyson's "*Princess*" than my "*Quaternions*." In saying all this I feel that I only echo what Wordsworth said to me while we were boating on Windermere in 1830 (I seem to see the splash of the oar). The words I do not presume nor pretend to repeat; but the spirit certainly was, that in Alfred Tennyson, young a poet as he then was, there was a man of the highest promise. I am, my dear Robert, your very old and very affectionate friend, William Rowan Hamilton.'

affections; and a few of them, it may be added, are so happy in thought and expression, as to claim their place in the poetry of his country. As not irrelevant to this topic, it is also to be remembered that he found in Mathematics a field for the exercise of Imagination, and thus exemplified the connexion, which he was in the habit of asserting, between the highest Mathematics and Poetry.

The same letter to Dr. Hart, from which I have just quoted, contains a reference to a recent correspondence of Hamilton's with Dr. Salmon, which may be given here as sufficiently summing up its purport to render unnecessary any further mention in this work of that correspondence. The passage has its value as a brief statement of steps in the history of double Algebra and Quaternions. I regret that the lucid exposition by which it is followed, contrasting the methods of these diverse systems, is too abstruse to be here inserted. It is certainly worthy of reproduction in connexion with the very valuable letter on the same subject to Professor De Morgan, prompted by the same incident, and which was written on the preceding 25th of June:—

'... In driving home yesterday I looked into the Second Edition (Paris, 1861) of Mourey's very ingenious little work. It was lucky that I could at once supply Salmon with a reference to the page of my Preface to the *Lectures*, in which I had cited the First Edition (Paris, 1828). But it is *foolish* to consider any such work as an *anticipation* of the quaternions. This brilliant and patriotic notion occurred lately to a French correspondent of our friend Salmon, who was so good as to send me the letter to read. The relation is rather of *contrast* than of *resemblance*, as in this very note to you I partly show. Systems which interpret + 1 differently cannot have much in common. I forget in what *year* it was that I first heard of Mourey; nor is it of the slightest importance. As long ago as 1829 my attention was called by John T. Graves to the work of Mr. Warren, published in Cambridge, in 1828, *On the Square Roots of Negative Quantities*. The systems of Warren and Mourey, *both* published in 1828, are substantially the same; though the Frenchman was livelier and smarter. What was

best in both had been anticipated in France by Argand, as I proved in the Preface above cited; and the notion of an *Algebra of the Plane* as connected with "*what we call Imaginaries*," had certainly been thrown out by Wallis, in the 17th century, in England, as the same Preface shows. De Morgan has since elaborated a system of what *he* (I think happily) calls "Double Algebra," on the same *basis* as others before him, but with many new *improvements* of his own.

'Carrying then back this *name*, I conceive myself to have been acquainted with Double Algebra, through Warren, from the year 1829. It cost me therefore *fourteen years to unlearn* that system, in order to pass from it to quaternions (in 1843). . . .'

On the 21st of August in this year Hamilton sent to Mr. Babbage a letter containing a full account of the system which he had invented some years before* for correspondence by cipher, and for which he claimed the merit of being able successfully to resist all attempts at deciphering without the use of the special key. Of this letter I have not been able to discover either draft or copy, but a large chart exists, made use of by the writer in preparing it. It seems expedient that this letter, if it can be recovered, should be placed, together with all connected documents, in the hands of some fit custodian.

Towards the close of this year some commotion was excited in the scientific world by the circulation, with a request for signatures, of a Declaration, the object of which was to meet doubts thrown by the votaries of the Natural Sciences upon the "Truth and Authenticity of Holy Scripture" by an assertion on the part of Christian men of Science of their faith in the perfect reconcilableness of the results of scientific investigation with the statements of the sacred record. Sir John Herschel communicated to the *Athenæum* his protest against this movement as interfering injuriously with the freedom of thought of scientific men, and as drawing a line of invidious distinction, of which the social effects would be

* *Supra*, p. 42.

pernicious; and in the same periodical Professor De Morgan, with powerful logical analysis, exposed what he stigmatized as the unfairness and sophistry of the Manifesto. Its social and logical faults having been thus dealt with by master hands, Hamilton, when subsequently applied to for his signature, sought to give his reply to the application, of which, equally with his friends, he disapproved, as compendious a statement as was possible, and put forward its want of authority as his sufficient ground of refusal. His letter to the secretary was as follows:—

'OBSERVATORY, NEAR DUBLIN,

'*December* 23, 1864.

'SIR,—I received yesterday a circular, posted (as it appears) by you in London on the day before, in which I am requested to sign a certain declaration, known for some months to scientific men. It may be from my living so much out of London that I am entirely uninformed by what authority it is sought to impose this Fortieth Article of Religion: which, with all reverence for the Sacred Scriptures, I must decline to subscribe. I have the honour to be, sir, your obedient servant.'

It may be admitted that the issuing of the Declaration was not ill-meant, but it will scarcely now be disputed that the measure was ill-judged. I refer the reader, in a note, to the parallel action, at a later time, of one well entitled to give, upon this subject, an influential opinion.* It is here sufficient to note that the part

* The decision arrived at in regard to this application by Herschel and Hamilton, men who were sincere Christians as well as eminent in science, is vindicated in what appears to me to be a very wise view of the point in question expressed by one who takes rank with them both as a scientific man and a Christian—James Clerk Maxwell—on the occasion of his being asked, in 1875, to join the Victoria Institute. I quote from his *Life*, p. 404, the rough draft of the commencement of his reply: 'I do not think it my duty to become a candidate for admission into the Victoria Institute. Among the objects of the Society are some of which I think very highly. I think men of Science, as well as other men, need to learn from Christ, and I think Christians whose minds are scientific are bound to study Science, that their view of the glory of God may be

taken by Hamilton in the incident furnishes additional proof of the sturdiness with which he valued intellectual freedom, and was determined to assert it for himself and his fellow-workers in the fields of Science and Philosophy.

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as extensive as their being is capable of. But I think that the results which each man arrives at in his attempts to harmonize his Science with his Christianity ought not to be regarded as having any significance except to the man himself, and to him only for a time, and should not receive the stamp of a Society. For it is of the nature of Science, especially of those branches of Science which are spreading into unknown regions, to be continually——' [Here the manuscript ends.]

CHAPTER L.

PROFESSOR CAYLEY. HAMILTON'S LATEST SCIENTIFIC WORK. LAST ILLNESS. HONOUR FROM AMERICA. DEATH.

(1865.)

WRITING on the second day of this, the last year of his life, Hamilton begins thus a letter to his younger son:—'It is a solemn thing, but I do not find it a painful one, to enter on a new year. I wish *you* many happy returns. It was my hope to have gone to Castleknock [to church] yesterday, but my cough was by no means so far gone as to make that safe.' These words may serve to indicate the religious ripeness of his spirit, and at the same time the shaken state of his bodily health, which from henceforth had to contend with a fatal combination of gout and bronchitis. To the same son, then serving as a curate at Clogher, he conveys his approbation of a sonnet, which on this account, and because of its upward-pointing significance, now specially congenial to the father's thoughts and feelings, I place in a note.*

* 'TO A LARK SINGING OVER SNOW.

Sweet bird, that nigh to Heaven's blue portal singest
Above the snow-clad Earth, thyself unseen,
And, free to roam where man not yet hath been,
Sweet hope to us from unknown fountains bringest,
A mild reproach to me thou downward flingest.
What share hast *thou* in brighter days to come?
This prison Earth must be thy lasting home,
Not that blue vault to which thou freely springest.
While thankless I, with idle hands, sit dumb,
Nor join the bird's glad song, the insect's hum;
While Heavenward my poor thoughts so seldom rise,
So faintly knock, so hardly enter there,
Thou hast the present entry of the skies,
But *I* the *Hope* that makes those realms so fair.

'ARCHIBALD HENRY HAMILTON.'

The following correspondence is upon an important mathematical theorem treated of by Hamilton in articles at the end of the *Elements of Quaternions*. I introduce it here mainly on account of its personal interest. It puts on record Dr. Salmon's judgment of what we may call the pœne-infallibility of Hamilton in mathematical calculation; Hamilton's real unwillingness to be a correcting critic of the work of another mathematician; his anxiety to do all justice to the powers and attainments of the great mathematician with whom he is thus brought into contact; his active care to guard the rights in discovery of Mac Cullagh, and to acknowledge the anticipation by Mac Cullagh of a theorem he had thought to be his own; his expressed obligation to Professor Jellett for enabling him to do this; his forwardness to make Mr. Cayley acquainted with all the facts; and, finally, the knightly disposition of the latter to accept the rectification.

The correspondence was begun by a letter from Dr. Salmon (Feb. 10, 1865), in which he writes:—

'. . . I believe you know I am printing a second edition of my book on surfaces. I have just added a note referring to your explanation of the lines of curvature through an umbilic on a quadric. I perceive these are valuable things which you have added to the theory of twisted curves. . . .'

Hamilton, in his reply (Feb. 13), after expressing his gratification, reports, in an important paragraph, his position in his own work (*Elements of Quaternions*):—

'I am at more last specimens of physical applications of quaternions, and am treating briefly of Fresnel's Wave in the half-sheet, 5 B. But I have *ready*, in advance, as another specimen of physical optics, an article on Mac Cullagh's Polar Plane: *after which I intend to devote scarcely more than a page to quaternion transformations of* $D_x^2 + D_y^2 + D_z^2$,* and so to *conclude* my applications.'

I have called this an important paragraph because, in con-

* The D in this expression I suppose to be the symbol introduced at p. 710 of the *Elements of Quaternions*.

nexion with what appears in his latest manuscript books, and in the final pages of the *Elements* as published, it proves that the last article in the book—that on the Polar Plane—was completed long before the article upon Fresnel's Wave, which precedes it, and upon which he continued to be engaged in the month preceding his death; and because we learn from the clause which I have italicised, what was the one remaining, but unpublished, physical application of quaternions, with which he would have completed this series of illustrations of his calculus, before winding up, by general concluding remarks,* his laborious and gigantic treatise. Immediately after the passage I have just quoted, he proceeds:—

'It was only last month that I was led to read Mr. Cayley's Paper in the *Proceedings* of our Academy, nor is the number beside me at this moment; but of course you can easily lay your hand on it, especially as *you* read the Paper for Mr. Cayley.

'My analysis does not confirm his enunciation of the "Principle of Equivalent Moments;" and I have been wishing to ask you two things:

'I. Was that principle or property enunciated by Mac Cullagh *himself*, or was it deduced by Mr. Cayley from his principles?

'II. Has Mr. Cayley ever *modified* his enunciation of it since the reading of the Paper?

'I found that the reflected vibration should be considered as at the end of the reflected ray in the *air;* Mr. C. places it at the end of a *prolongation* of that ray in the *crystal.* This does not at all touch the *mathematical* merit of the Paper; but it would make the greatest difference physically, in any consequences to be drawn respecting planes of polarization, &c. In short, I express the principle by *an equation of three terms*, and differ from Cayley by the *sign* of *one* of them.'

Dr. Salmon, in his reply (Feb. 24, 1865), writes:—

'. . . I have forgotten all about the polar plane. I was writing to Cayley, and sent him on your criticism. I happened to

* See *Elements*, pp. xiii., 495, 496.

mention to a friend yesterday, as an illustration of the amount of my faith in you, "Sir William Hamilton writes me word that there is an error in a result of Cayley's, and though he expresses himself with all possible modesty as to the possibility of there being an error or misconception on his side, still, without knowing anything more, I believe firmly that there is an error as he alleges. And if Sir William were to tell me that a demonstration of my own was erroneous, I fear I should have no firm faith in any demonstration of mine till his objection was withdrawn."'

On the 4th of March, Hamilton to Salmon continues thus:—

'I assure you that it is with great regret I find myself differing in *any* point from Mr. Cayley, my admiration for whom has certainly sustained *no diminution* since I published, in 1847, my "First Series of Researches respecting Quaternions," in vol. xxi. part ii. of the *Transactions* of our Academy. In the last page of Note A, at the end of that Series or Essay, I expressed myself as follows (with respect to an early application by Mr. Cayley of Quaternions to the rotation of a solid body): "That important application of the author's principles had indeed occurred to himself previously; but he was happy to see it handled by one so well versed as Mr. Cayley is in the theory of such rotation, and possessing such entire command of the resources of algebra and of geometry." Nevertheless, having gone over my calculations again, and with great care, during the last few days, I cannot *now* feel *any doubt* of the existence of an oversight in the Paper which I mentioned last month. It was read by you (I remember that I could not attend that evening) on the 23rd of February, 1857, for Mr. Cayley, and is printed in vol. vi. part iv. of the *Proceedings* of the Royal Irish Academy. I believe that I told you that I found the *spherical trigonometry* to be *all right*, when reading the Paper for the first time in January last, but that I objected to the *sign* of the *moment* of the *reflected vibration*, as given in page 491 of the Part above cited. Recent calculations confirm this result; but they go much further, for they show that when the reflected vibration is placed at the end of the *reflected ray* in air, the "Principle of Equivalent Moments" holds good for *every axis* which is *parallel to the face of the crystal*.'

Not having gained from Dr. Salmon the historical information

for which he sought, he turned, on the 6th of March, to Professor Jellett:—

'If you have received certain proof-sheets, which I indulged myself by sending to you recently, and if you will turn to the note at the foot of page 736, you will see that I have announced an intention of deducing, by quaternions, Mac Cullagh's "Theorem of the Polar Plane:" and in fact I have to-day made up for the printers, and am now sending to the post, my "Series 423," in which that deduction is given, with what seems to *me* great simplicity. But as I have also sketched a short *addition* to the *Series*, which I am likely to send to-morrow, though it cannot well be in type this week, and which contains what appears to me a *new theorem of physical optics*—the principles of Mac Cullagh and Neumann being used in the quaternion deduction—I am anxious to consult you on the few following points:—

'I. Was the principle or property, called by Mr. Cayley the *Principle of Equivalent Moments* [*Proceedings*, R. I. A., vol. VI. p. 481, &c.], a *deduction* of *Mr. Cayley's own* from Mac Cullagh's principles, by him referred to?

'II. Or had the "principle of equivalent moments," as enunciated by Mr. Cayley, been actually deduced by *Mac Cullagh himself* (Mr. Cayley's modest tone *suggests* the latter alternative, but I do not know where to find any statement by Mac Cullagh on the subject)?

'III. Have *you*, since, either *confirmed* or *corrected* the enunciation of the principle in question (given at p. 489 of the Paper)?

'IV. If not, do you know of *anyone else* having done *either*? For agreement with my "*Theorem* (or Principle?) *of the Resultant Couple*"—rather recently arrived at—the enunciation *requires* to be *corrected* by what amounts to a *change of sign* of the *reflected vibration;* or better, by considering the *reflected ray in air*, and *not* its prolongation in the crystal.

'But even *after this correction*—which falls chiefly on the *sign* of the *moment*, given at top of page 491—the principle treated of by Mr. Cayley is only a *very particular case* of my new theorem, above referred to, of which I shall be happy to send you the statement, translated from the new quaternion formula."

Professor Jellett's reply, dated March 9, was as follows:—

'The principle of the equivalence of moments is expressly stated by Mac Cullagh as a part of the general theorem by which he solved the problem of crystallized reflexion and refraction. You will find it in the twenty-first vol. of the *Transactions*, pp. 41–2.

'I confess that my interest in his whole theory has been much lessened by the discoveries of Jamin, which show that the theory can at best be accepted as only approximate, and in certain cases not even that.

'I have not paid any attention to the subject—at least to Mac Cullagh's mode of handling it—for some time, and must therefore answer III. and IV. in the negative.

'But I am not quite sure whether the verification you speak of be or be not experimental.'

As his winding up of the correspondence Hamilton transmitted through Professor Jellett to Dr. Salmon, and through the latter to Professor Cayley, a manuscript-sheet containing a transcript of the last printed sub-article in the text, viz. (12), and the two notes appended on pages 761 and 762, of the *Elements of Quaternions*. In the second of these notes he refers to Mr. Cayley's "very clear and able Memoir," and after mentioning the "slight inadvertence in a Paper of such interest and value," repeats his "admiration (long since publicly expressed by him) which is due to the vast attainments of a mathematician so eminent as Professor Cayley." To this transcript was added, with the date March 13, 1865, the following note addressed to Dr. Salmon:—

'I hope that I shall be considered as not deficient in courtesy to Mr. Cayley, to whom, if you choose, you can forward this sheet. A letter received this morning from Jellett has given me a most useful reference to a later Paper by Mac Cullagh, in which I find myself supported, but of course anticipated, in what I had thought my own "Theorem of the Resultant Couple." So if I differ from Mr. Cayley I have Mac Cullagh on my side. My investigation was naturally quaternionic throughout; and I have not cancelled any part of it, but merely made an *Addition*, as above, to the last

note of the series (423), which had already been sent to the printers, but will scarcely reach me in type before next week.'

The sheet was transmitted by Dr. Salmon to Professor Cayley, by whom it was thus acknowledged in a letter addressed directly to Hamilton:—

'CAMBRIDGE, *April* 3, 1865.

'I return with thanks the enclosed Paper, forwarded to me by Dr. Salmon. I can have no possible objection to the publication of it. I do not understand you to say that there is any error of sign in my theorem (2), but it is very probable that the true interpretation of this in reference to Mac Cullagh's theory should have been

$$-Rt\cos RU\sin NUR + R''t''\cos R''U''\sin NU''R''$$
$$= R't'\frac{\sin NW}{\cos WR'\sin NR}\cos R'U'\sin NU'R';$$

– instead of + in the first term, and that in consequence I have wrongly enunciated the Principle of Equivalent Moments. I am not able to put myself back into the question to see that this is so, nor do I remember where my enunciation of the principle was taken from: I did not attend to the theorem otherwise than in a geometrical point of view, and was satisfied by obtaining the theorem (2) as a theorem in pure geometry, the interpretation of which should be the principle in question. I am therefore quite willing to admit that your correction is right.'

The transcript, above mentioned, contains the whole of the concluding pages, from p. 760 (12) of the *Elements of Quaternions*, with the exception of the short sentence with which the last note concludes. This addition was founded upon the information which Professor Jellett's letter imparted. It is to be regretted that in preparing the manuscript for the press the latest form of this sentence was overlooked, as well as important following clauses, the main object of which was to obviate a possible inference from it to the disadvantage of Mac Cullagh. The passage, as finally settled, runs thus:—*

* See Manuscript Book D. 1864, p. 117.

'The writer understands that subsequent experiments by Jamin and others are considered to diminish the *physical value* of the theory above discussed. But this neither detracts on the one side from the great intellectual and physical genius of Mac Cullagh, nor renders inappropriate on the other side, for the purposes of the present work, the recent pendant to the quaternion expression of Fresnel's earlier views.' *

It is right I should here note that in the autumn of 1864 Hamilton commenced a series of memoirs characterized by the use of determinants, which was carried on far into this the last year of his life.†

On the 24th of March—a date elsewhere‡ assigned for another piece of work—Hamilton received a letter from Dr. Charles Graves, at this time Senior Fellow of Trinity College, and Dean of the Chapel Royal, on the subject of Binomial Co-efficients. Dean Graves wrote:—

'You know that the sums of the alternate coefficients in the binomial development are equal. . . . But is the corresponding theorem known respecting the three sums obtained by taking every *third* term in the series

$$1,\ n,\ \frac{n\,(n-1)}{1.\,2},\quad \frac{n\,(n-1)\,(n-2)}{1.\,2.\,3},\ \&c.?$$

You will find that two of these are equal and the third differs from them by unity. The more general theorems are less elegant.'

On the very day of the receipt of this inquiry Hamilton replied:—

'Your *stated* theorem was quite new to me; I need not say that the *suggested* theorems were also unknown. But I have this

* See Appendix for additions to *Contents* of *Elements of Quaternions.*

† See Manuscript Book E., 1864, pp. 11 . . . 40, 101 . . . 116, 117, 119. . . . A *précis* of these memoirs is given in the Appendix.

‡ See Appendix: 'Memoirs in which Determinants are used.'

morning had the pleasure of proving the theorem which you enunciated by assigning general expressions for the three *separate sums* to which it refers. And I think that the analysis (by imaginaries and determinants) which I employed ought to extend to sets of four coefficients,' &c.

On the next day he sends his proof of his friend's theorem, and on the 1st of April a generalisation, embracing all similar theorems. Dean Graves's theorem and proof, and Hamilton's generalisation, were subsequently communicated to the Royal Irish Academy, and may be found printed in its *Proceedings* for the meeting of June 26th, 1865. In the same number of the *Proceedings* is contained Hamilton's last addition to his long list of contributions to the scientific memoirs published by the Academy, of which he had been a member almost from his boyhood. It is entitled 'On a New System of Two General Equations of Curvature . . . all deduced from Gauss's 2nd method in his *Disquisitiones generales circa Superficies Curvas*,' and has appended to it 'Three Verifications of Measure of Curvature.' *

Soon, however, Hamilton's wonderful activity of intellect, up to this time manifested uninterruptedly both in the carrying forward towards completion of the *Elements of Quaternions*, and in digressive exercitations suggested by sympathy in the work of friends, was brought to a pause by a severe attack of the illness from which no entire recovery was granted to him.

* This Paper is additional to his treatment of the subject published in the *Elements*, §§ 412, 413, and indeed is in continuation of a very extensive investigation commenced on the 11th of January, 1864, and carried on at various dates up to April 25, 1864, as recorded in two folio manuscript-books (1864, No. 1, and 1864, No. 4), with the titles following:—'Lines of Curvature and Curvatures of Surfaces, partly by Quaternions, partly by the methods of Monge and Dupin,' 38 pages, 130 articles. 'Gauss's Measure of Curvature of a Surface,' 2 pages, 11 articles. 'Intersections of Normals to Quadrics,' 74 pages, 262 articles. 'Correspondence with Dr. Hart,' 13 pages. A third similar folio manuscript book (48 pages, 224 articles) is filled by an investigation, both by determinants and quaternions, of the 'Locus of the Vertex of a Quadric Cone having Six-point contact with a Curve in Space,' with successive dates of work from February 1, 1864, to August 7, 1864.

On the 26th of April, 1865, we find him thus writing to a friend:—

'. . . It is very kind of you to inquire about my health and spirits, and to tell me something of your own. Mine have not been *good* for (I may say) some *years* past; but I am working away as usual. So much work produces naturally fatigue: in fact, it has injured my constitution, at least for the present; and it is not particularly conducive to good spirits to find that I have been running up what is (for my purse) a rather heavy bill with my printer.* But I have many blessings, and do not wish to be considered as a grumbler. . . .'

On the 9th of May, Hamilton was in Dublin for the last time. On that day was opened the International Exhibition held in the Building now occupied by the Royal University of Ireland. It excited great interest in Hamilton: so much so that he declared his intention of coming in frequently to study its contents. But

* In reference to this subject I grieve to say that it remained a weight upon his spirits up to the day of his death. On the 30th of May, 1865, the University Printer sent him an account, showing that the cost of paper and printing of the *Elements of Quaternions*, up to that date, was £395 12*s*. 1*d*. Towards the discharge of this sum £200 had been paid—£150 by the Bursar of Trinity College, and £50 from Hamilton's private purse—thus leaving a balance against the still uncompleted book of £195 12*s*. 1*d*. There remained, however, £50 still unexpended from College grants already sanctioned, so that £145 was the sum still chargeable against Hamilton for the past; and to this an addition would have to be made on account of work subsequent to May 30. After his death, the Board of Trinity College liberally paid the above balance, together with the accrued addition just mentioned, and had then to defray the expense of binding, advertising, &c. The whole expense of printing and publishing amounted to close upon £500; and as the impression consisted of only 500 copies, many of which were presented to men of science, and scientific bodies—although the book was soon out of print—no profit could have resulted from its publication at the price of £1. I understand that so high a sum as £5 has been given at a public sale for a single copy. For verification of these details I refer to Manuscript Book R, 1865, pp. 25-28. It contains the copy of a letter from Hamilton to the Bursar of Trinity College, stating the above facts, and dated August 22, 1865—a date less than a fortnight distant from his death. One clause in this letter was, 'You will see that I have already paid him [the University Printer] Fifty Pounds of my own money—a not very pleasant operation.'

this was not to be. An attack of acute gout in the lower limbs rendered it necessary for him to summon, on the 13th of May, the aid of Dr. Wyse, who continued to be in almost daily attendance for the remainder of the month, and who, as Hamilton's daughter records, was greatly struck by his patience under unusually severe suffering. It would seem that on the first three days of June he was apparently better, for the physician's visits were discontinued; but on the 4th, alarming symptoms were manifested, and Dr. Stokes had to be sent for. The measures adopted were not sufficient to ward off an aggravated seizure on the following day, which took the form of epileptic convulsions. These were most severe, threatening immediate and mortal collapse. They were, however, under the direction of Dr. Stokes, at length subdued, leaving his strength finally shattered, and his mental powers for a time disabled. Before the end of the month he was again at work at his book, and corrected for the press his Papers for the *Proceedings* of the Academy; but soon bronchitis supervened, and, with other ailments, led on to the inevitable close.

It should not be here omitted that, during the continuance of this alarming and disabling illness, Hamilton and his family received from their neighbours, Mr. and Miss Rathborne, practical and effective sympathy of the most valuable kind. The two sisters of Lady Hamilton had before this time passed away.* Scripplestown was now occupied by new inhabitants, but Dunsinea was still a family possession; and Mr. John Garnett Rathborne succeeding to his father, Mr. Henry Rathborne, proved always an attentive and attached neighbour to his aunt and her husband. His sister, Kate Rathborne, was the niece to whom, in the days of her opening beauty, Hamilton had addressed verses, printed in the second volume of this work.† Her intelligence and character developed with her years, and she became to her uncle a favourite relative, regarded by him with constant interest and affection.

It was just at this crisis that a letter reached him from across

* See vol. ii., p. 2.

† See vol. ii., pp. 61, 273.

the Atlantic, which shed brightness over his decline, and made him feel that, whatever might be the issue of his illness, his early dream of world-wide recognition was realised. It is remarkable that the land which first from a distance hailed with generous encouragement the beginning of his brilliant career was now to confer upon him a final crown of honour. When he was only twenty-seven, he received a diploma constituting him a Fellow of the American Academy of Arts and Sciences;* now, when concluding his sixtieth year, and battling with mortal disease, he receives from the same great country the highest scientific honour in their power to bestow. It was after the tremendous conflict of North and South was ended, and both divisions of the continent had been by it welded into one nation, that the first truly national Academy of Science in America was formed. One of its earliest duties was to draw up a list of the most eminent scientific men throughout the world, upon whom the honour of Foreign Associate should be conferred. A discussion took place as to what name should be first on this list, and by a two-thirds majority it was decided that that place should be occupied by the name of William Rowan Hamilton. I give the letters of Mr. Gould, containing the details. I am sure that to the scientific chiefs of that noble nation it must be ever a deep gratification that the act of their Academy was in time to be welcomed by the dying mathematician, and they will learn with interest that the last letter written by him was in acknowledgment of it. It will be seen, indeed, that this letter, written only a week before his death, was a very inadequate acknowledgment of the distinction he had received; but it was all his failing powers were capable of, and it intimated his intention of more fully expressing his feelings. A previous letter to Dr. Hart shows that those feelings were more than feelings of mere gratitude: that he was deeply impressed by the honour. Dr. Salmon's letter, which I subjoin, must have afforded him additional gratification, as expressing personal sympathy, participated

* See vol. i., p. 610.

by many scientific and private friends, and the distinguished writer's sense of the honour reflected on the University of Dublin.

From B. A. GOULD *to* SIR W. R. HAMILTON.

'CAMBRIDGE, MASSACHUSETTS, *May* 17, 1865.

'DEAR SIR—In the absence of Professor Agassiz from the country, the agreeable duty devolves upon me of announcing to you that the National Academy of Sciences, established by the United States on the 3rd of March, 1863, elected you, on the 9th of January following, first on the list of its Foreign Associates, now fifteen in number.

'As no reply has been received to the notification directed by the Academy a year ago, it is feared that it may have failed to reach you; and I therefore have the honour of addressing to you this duplicate announcement. A diploma will be transmitted hereafter.

'The annual Reports to Congress for 1863 and 1864, and the Academy's Annual for 1865, are this day sent you through the Smithsonian Institution.

'Allow me to add the expression of my personal gratification at the honour which the Academy has done itself in placing your name at the head of its foreign list, and of our hope for your cordial sympathy with our efforts to organize now, for the first time, a National Academy in the United States.

'I am, my dear sir, very respectfully yours,

'B. A. GOULD,

'*Foreign Secretary pro tem.*'

From the SAME *to the* SAME.

'CAMBRIDGE, MASSACHUSETTS, *May* 20, 1865.

'MY DEAR SIR—In August or September next it will have been twenty years since—within ten days after first setting foot on European soil—I found my way to you, and was received with a cordial welcome and kind hospitality, which might well have gratified a man of established scientific fame, instead of a youngster under twenty, who had never seen much of the world on this side, or any of it on your side the Atlantic.

'I cannot tell how often my memory has reverted since then to your beautiful park and your pleasant house and cordial greeting, and hoped that it might yet be my good fortune to pay my respects to you once more. The lapse of time seems, however, to make it more difficult with each successive year to leave the various duties which seem to bind us closer and closer to our homes and domestic routine of duties, and it is doubtful whether I may ever have the hoped-for gratification.

'But I cannot resist the impulse to send these few lines while waiting the accompanying official document, and to express my hope for the long continuance of your health and scientific activity.

'Our new Academy is the first national or governmental institution we have ever had in the United States. All other scientific societies have been purely voluntary associations. This one was founded in the midst of the severest trials to which a nation was ever subjected without destruction; and it is not the least of our sources of pride and hopefulness in its behalf, that it dates from such an epoch and crisis in our country's existence. You can easily imagine that the difficulties to be overcome in the United States are very different, as well as very much more serious, than those which would be encountered in an older country or in one whose form of government gave the chief power to the most cultivated and highly educated class. It is to the representatives of the people that we must look for support and aid; and these are, in general, just what their name indicates—representatives of the people, neither better nor worse than the average. But as they all mean to do in these matters what is best for the whole country, it is only needful to convince them of what is requisite, though this is not always an easy matter.

'In January, 1864, the Academy voted to elect ten Foreign Associates, and accordingly twenty names were agreed on; after which the discussion turned, not on the election or non-election of any one of them, but upon the order in which they should be inscribed upon the rolls. It was soon narrowed down into a discussion as to whether your name or that of Professor von Baer, of St. Petersburgh, should head our list; and the Academy finally decided the question by a vote of two-thirds against one. Whether the notifications then sent by Professor Agassiz were lost or mislaid, or suffered shipwreck, we cannot learn. But they have none

of them apparently reached their destination: certainly no responses have been received, and Professors Argelander, Milne-Edwards, and Brown have certainly not received those sent to them. Therefore they are all sent anew by the present mail.

'Please accept my sincere respects and best wishes for your continued health and usefulness, and believe me, my dear sir, ever most faithfully yours.'

From SIR W. R. HAMILTON *to* A. S. HART, LL.D., S.F.T.C.D.

'OBSERVATORY, *June* 13, 1865.

'MY DEAR DR. HART—It was not until *to-day* that I *read* an official letter from America, which reached me more than a week ago, and which I now enclose for your perusal, and, if you think that it could interest them, for that of the Board also. To have been elected last year out of the whole world by the new National Academy of America, and, as a private letter informs me, by a majority of two to one, the *first* of its Foreign Associates appears to me so surprising a thing, that I might be apt to treat it as incredible, if I had not been long acquainted with the writer of the communication.

'To the indulgence of my College patrons the result may appear less extravagant than to myself. I shall wish to have the letter returned at your convenience.

'I have still papers connected with my book to enclose to you, but for the moment can only subscribe myself, dear Dr. Hart, very truly yours.

'P.S.—I am considered to be slowly recovering, and am able to work a little.'

From GEORGE SALMON, D.D. *to* SIR W. R. HAMILTON.

'T. C. D., *June* 16, 1865.

'MY DEAR SIR WILLIAM—I have heard with very great pleasure of the honour done you, and through you to our University, by the National Academy of America. I am the less surprised, however, as I had heard some years ago from Professor Peirce of the extent to which your *Lectures on Quaternions* circulated and were appreciated in America.

'If this day week (Thursday) should be fine, I should like to go out to see you. As, however, I only intended a friendly visit of inquiry, do not have the least scruple in telling me if, when the time comes nearer, you do not feel yourself quite equal to receiving visitors. If you were living in town we should, no doubt, all be calling every day to ask how you were going on. But, as you are not so accessible, it is natural to prefer to make my inquiries by letter, unless there is a hope of seeing you. I remain very sincerely yours.'

From Sir W. R. Hamilton *to* B. A. Gould.

[from a copy.]

'Observatory, *August* 24, 1865.

'My dear Mr. Gould—I have been prevented by illness from writing sooner to acknowledge the high compliment paid to me lately in America, and announced in your letter. I am anxious not to let another post pass without doing so, and shall write afterwards more fully. I am, &c.'

On the 16th of July I went out to the Observatory to visit my old friend. I had not seen him since the 4th of May. I had heard of his being present at the opening of the International Exhibition, but the tidings of his severe illness was slow in reaching me. A false rumour, indeed, of his having been again in town led me to invite him to meet my brother-in-law, Leopold von Ranke (then in Dublin to receive from our University an honorary degree). The contents of his reply, written 'before daybreak' on July 4, and the infirm handwriting, undeceived me as to his condition; but illness of my own and incessant occupation prevented me from making earlier personal inquiry. I was greatly shocked by the change which had taken place in the interval. Emaciated and feeble, he seemed altogether in physical respects a different man. His intellect, I soon found, was as clear and active, and powerful as ever, and I was deeply impressed by the gracious sweetness, gentleness, and humility, which shone through his manner and every word he uttered. He spoke with thankful-

ness of his having been brought through the struggle for life which he had undergone, and of the expectation thus revived that the great task he had on hand would very soon be accomplished by his sending to the press the last sheets of his *Elements of Quaternions*. And looking beyond this event, he intimated his intention of turning afterwards, for refreshment and variety, to the study of poetry and to the putting down of his thoughts on metaphysics. I spent with him nearly four hours in conversation, embracing the higher topics of the day, religion included, part of the time in his beloved garden, part in the house, from which with kind consideration he insisted on sending me homeward in his car; and I felt, as I drove away that summer evening from the Observatory, that never in the long period of our friendship had my feeling towards him been one of deeper admiration and affection.

On the 5th of August Hamilton commenced what was intended to be a short series of letters on Quaternions, addressed to his younger son, by writing with a tremulous hand the letter describing the circumstances of his great discovery which has been printed in the second volume of this work.* An introductory sentence to a second letter of the series is all that he was able afterwards to accomplish. In the Appendix of this volume will be found some paragraphs carrying on the *Contents* of his book, which were written towards the close of the month. The 26th of August is the date attached to the last of these paragraphs, and this, I believe, is his latest registering of scientific work. On the 22nd he wrote to the Bursar of Trinity College his letter already referred to,† respecting the University printer's account, and on the 24th of August to Mr. Gould his brief acknowledgment of the honour wafted to him over the Atlantic. These were his final efforts.

On the 2nd of September, in response to a summons from his eldest son, I proceeded from Howth, where I was then staying, to the Observatory. I was met at the door by Lady Hamilton in

* Vol. ii., p. 434.

† *Supra*, p. 202.

tears, and from her and Mrs. Joseph Willey, who was rendering valuable aid as nurse to her relative, and from his eldest son* I heard the sad tidings that the end was at hand. I found my friend in bed, his breathing rendered difficult by bronchitis, but his mind calm and in its full strength. He at once disclosed his consciousness that he was approaching the termination of his life. After referring to his long friendship with myself and other members of my family he spoke on general topics of a religious bearing, taking occasion, as I have already intimated,† to express his disapproval of Bishop Colenso's treatment of Abraham's interrupted Sacrifice of Isaac, and expressing his conviction that God, as supreme Lord of life, able to give, to take away, and to restore, might quite justifiably give the command impugned: though he considered not unlikely, what I urged, that the whole transaction, besides being a test of Abraham's faith, might be rightly interpreted as a process of symbolical instruction, one lesson of which, though not the highest, was that human sacrifice was *not* acceptable to God as part of human worship. Turning to his own relation to God, he asked me a question by which I was deeply moved, manifesting, as it seemed to me, his humble searching of heart, 'Did I think that God could love him?' I replied as the Christian minister has the happiness of being able to reply to such inquiry from a God-loving, yet self-arraigning, self-condemning, spirit, and he was satisfied. He then asked me to pray with and for him, telling me that he had found in the 145th Psalm, which he had asked Mrs. Willey to read for him, a wonderfully suitable expression of his thoughts and feelings—and truly in that Psalm we may read his admiring thoughts of God and God's works, his feelings of gratitude for the mercy that had sustained him through life, upheld him in falls, and listened to his cry in sorrow. I complied with his desire; after which he said that he wished to testify

* His younger son and his daughter were now at Clogher: the former serving his cure, the latter recruiting her strength after illness.

† Vol. i., p. 179.

his faith and thankfulness as a Christian by partaking of the Lord's Supper. He added words to this effect—that, personally, he would rather receive it at my hands, or those of his son, than from anyone else in the world, but that he thought the rubrical direction of our Church should be observed (thus manifesting at the last his deeply-seated respect for order and law), and that he would therefore ask me to request his Parish Clergyman, Dr. Sadleir, to come to the Observatory on the next day, if possible, for the purpose of administering the sacred rite, and that I would join him in partaking of it. I, of course, consented, and withdrew, as the arrival of Dr. Stokes and Dr. Wyse was announced. He then roused himself and used for the last time his pen—so long his almost inseparable instrument of thought—in feebly writing a few words to prepare his physicians for finding him with little voice left after his long converse with me. Upon their coming down from his room I gathered from them that he was indeed come to the final stage of his illness, but that it could not be pronounced how long his powers would hold out. Dr. Stokes, who was at the time my medical adviser, would not suffer me to remain, as I wished to do, but insisted on taking me back with him to Dublin. I therefore went again to my friend's bedside, and was struck by the signs of a great collapse of vital energy, but had the satisfaction of a momentary interchange of farewells. Very shortly after our departure, at 2h. 30m. p.m., he breathed his last, having first, as I learned the following day, solemnly stretched himself at his full length upon his bed, and symmetrically disposed his arms and hands, thus calmly to await his death.

News of the event reached me the next morning, accompanied with a request, immediately acted upon, that I should revisit the mourning family. I then beheld, for the last time, the form of my friend, and I shall never lose the impression of grandeur and majesty made upon me by the noble head and the monumental figure. More than once, in after times, Dr. Stokes has said to me that in all his long experience he had never seen a human being

whose aspect, under the subliming power of death, could compare, in the attributes I have named, with that of Hamilton.

The Provost of Trinity College, Dr. Richard Mac Donnell, writing on the 4th of September, expressed, in the following words of a letter to Hamilton's son, his estimate of the event viewed in connexion with the history of the University of Dublin: 'Accept my heartfelt condolence for the great loss which your family has sustained. The loss to our University is irreparable, for since the creation of our University we have not had so distinguished a member of our body.'

The funeral took place on the 7th of September. His remains were brought from the Observatory to the Chapel of Trinity College, where the first part of the Burial Service was read by his friend, Doctor Todd, Senior Fellow; thence a procession followed the hearse to the cemetery at Mount Jerome. The time of year accounted for the absence from Dublin of very many who would have joined it. I may name two who were, with great regret, at a disabling distance—his early friend and pupil, the Earl of Dunraven, and Dean Graves. Still great numbers formed a procession, which included, besides his family and connexions, a large body of College students, headed by the University authorities; many citizens of Dublin, preceded by the chief officers of the Corporation of his native city; and members of the Royal Irish Academy, attended by their secretary, Mr. Clibborn, bearing the Academic mace veiled in mourning reminiscence of their great President. I might here give a list, not uninteresting, of eminent men who paid this tribute of their respect, but must content myself with mentioning that among them were Hamilton's old friends, Dr. Hart, Dr. Salmon, Dr. Stokes, Dr. Petrie, Sir Thomas Larcom, Denis Florence Mac Carthy, and Professor Adams of Cambridge. The conclusion of the Burial Service was read by Dr. Sadleir, Incumbent of Castleknock. The grave appropriated to Hamilton, on the north side of the cemetery, is marked by a headstone, subsequently erected by his family, and bearing the inscription:—

HERE LIE THE MORTAL REMAINS OF

SIR WILLIAM ROWAN HAMILTON, LL.D.,

ROYAL ASTRONOMER OF IRELAND.

HE WAS BORN AUG. 4 : 1805.
HE DIED SEP. 2 : 1865

'— IN THE LOVE OF GOD, LOOKING FOR THE MERCY OF OUR LORD JESUS CHRIST UNTO ETERNAL LIFE.'—*Jude* 21.

It should here be recorded that the Civil List Pension of £200, which had been conferred on Sir W. R. Hamilton in 1843, was continued, after his death, to his widow and daughter conjointly. Lady Hamilton died in 1869, his daughter, Mrs. O'Regan, in 1870, when the pension lapsed. An effort was then made to obtain a grant from the same source for his only surviving sister, Sydney Margaret Hamilton, a lady who would in every respect have been a fitting recipient of the favour—for she had in many ways aided her brother, both in domestic emergencies, and in the routine work of the Observatory, was of exemplary character, and was at this time in very narrow circumstances. But though a memorial, urging the grant, was signed by the chief men of science and literature in Ireland, the Premier of the time, Mr. Gladstone, found himself unable to accede to its prayer. Miss Hamilton afterwards emigrated to New Zealand, where she still lives, esteemed and honoured. The elder son of Sir William, Mr. W. E. Hamilton, has become a citizen of the Dominion of Canada, and the younger son is a laborious and respected, but unbeneficed, clergyman in the North of Ireland. Sir William's only grandchild, John Rowan Hamilton O'Regan, born but a few weeks before the death of his mother, is now a promising scholar of Clifton College.

CHAPTER LI.

CHARACTERISATIONS OF HAMILTON AND OF HIS WORK.

THE family of Sir William Hamilton received in their affliction many letters of condolence. Two of these, addressed to his widow, are here inserted, inasmuch as the writers, Sir John Herschel and Augustus De Morgan, were eminently qualified to form an adequate judgment of the intellect, the scientific work, and the character of the friend whom they had lost, and it would be unfair, I think, both to Hamilton and to the public not to place that judgment on record.

From SIR J. F. W. HERSCHEL *to* LADY HAMILTON.

'COLLINGWOOD, *September* 14, 1865.

'DEAR MADAM—I trust that, though personally a stranger, I shall not be thought obstrusive in offering my most sincere and heartfelt condolence on the occasion of your late heavy affliction, as one who loved and admired him whom it has pleased Providence to remove (while yet at an age when his country and science might still have expected so much from him) with no common love and admiration.

'The event, which I learned only from its public mention in the papers, struck me with the more painful surprise, as I had not heard of his previous illness, and have been lately several times on the point of writing to inquire respecting the progress of that really important work on which I knew that he was engaged—an exposition, in a form adapted to the comprehension of ordinary mathematicians, of the principles of his wonderful *Calculus of Quaternions*. I trust that he left this work, if not quite completed for press, at least so far finished that it may yet see the light. I will not intrude longer on your sorrow. Permit me only to add that among the many scientific friends whom time has

deprived me of, there has been none whom I more deeply lament, not only for his splendid talents, but for the excellence of his disposition and the perfect simplicity of his manners—so great and yet so devoid of pretension—and allow me to remain, dear madam, your faithful servant, J. F. W. Herschel.

'P. S.—I need not add that Lady Herschel desires me to add the expression of her hearty condolence.'

From Professor De Morgan *to* Lady Hamilton.

'91, Adelaide-road, N.W., *September* 5, 1865.

'Dear Lady Hamilton—I shall certainly not intrude on your grief by sending this letter according to its date; I write it on the day on which I have received the shock of hearing that one of my dearest friends has passed away, without my knowing that any the least alteration had taken place in his health. That I shall sooner or later offer you my heartfelt sympathy is a thing of course; and I cannot do it better than by writing at once, though I know the sending it at once would be wrong.

'Several months have passed since I heard from your husband. This was not uncommon; our correspondence was often interrupted by longer periods; it went and came by fits and starts. I hoped that all things were going on as usual, and was meditating a letter of inquiry to be replied to as many of the same kind had been before, when three lines in the *Daily Telegraph* put me in possession of the sad news that nothing of my friend was left in this world except his deathless fame and his mourning family.

'I have called him one of my dearest friends, and most truly; for I know not how much longer than twenty-five years we have been in intimate correspondence, of most friendly agreement or disagreement, of most cordial interest in each other. And yet we did not know each others' faces. I met him, about 1830, at Babbage's breakfast-table, and there, for the only time in our lives, we conversed. I saw him, a long way off, at the dinner given to Herschel (about 1838) on his return from the Cape; and there we were not near enough, nor, on that crowded day, could we get near enough, to exchange a word. And this is all I ever saw, and, so it has pleased God, all I shall see in this world, of a man whose friendly communications were

among my greatest social enjoyments and greatest intellectual treats.

'There is not a word which I could offer to you or yours, on any of those considerations which bring such comfort as the case allows, other than what must suggest itself, and must be better enforced by those around you. That you should soon find comfort in such considerations would be—nay, *is*—his chief wish for you and your children. May you find it!

'The time will come when you, or Miss Helen, will feel able to write me something about his last months, his decline, and the state in which his scientific matters are left. You will, I sincerely hope, not hurry in compliance with my request. No number of months will abate my interest in the matter. I have watched his career from the beginning. When I—a year younger than himself, as it happens—was an undergraduate not far advanced, and he must have been about nineteen years old, I heard of the extraordinary attainments of a very young student of Trinity College, which were noised about at Cambridge. This rumour was made more interesting by other rumours which also circulated about the same time concerning another young Irishman, then recently matriculated at Cambridge. This was poor Murphy, whose subsequent career, though great in mathematics, fell short in conduct and discretion. He wanted all but mathematical education in early youth. The appearance of the two at once in the field gave both an interest, and I was thus led to watch Hamilton's career before I knew anything of him personally.

'His memory will be very bright and very lasting. I trust that care will be taken to illustrate the singular variety of his attainments and the fertility of his mind. His publications give no more than a glimpse of what he was out of mathematics. Nor must it be left unrecorded how truly good he was as a man and a member of Society. In exact science he will be the Irishman of his day and of that to come, just as much as his namesake was in mental speculation the Scotchman.

'With the warmest wishes for you and yours, I am, dear Lady Hamilton, sincerely yours, A. De Morgan.'

In a brief memoir of his friend, printed in the *Gentleman's Magazine* for January, 1866, Professor De Morgan bears additional

testimony to the moral and intellectual character of Hamilton, and furnishes some illustrative anecdotes and details which have a value, as helping to produce a life-like image of the man. The following are extracts:—

'Hamilton was a man who combined different talents to an extent which is often attributed, by exaggeration, to the possessor of one powerful faculty; but in his case there is abundant evidence. He was scholar, poet, metaphysician, mathematician, and natural philosopher. Highly imaginative and fluent of tongue, he was an orator in all that he knew; even in mathematics, to the details of which he could give almost a rhetorical cast in a letter. In metaphysics he was very well read, and could talk in a way which suggested to Southey a comparison and a difference. Hamilton one day preached to Southey on this subject, until the latter remarked, as they passed a ploughman, "If you had been Coleridge, you would have talked to that ploughman just as you have been talking to me...."

'Hamilton was not only an Irishman, but Irish; and this with curious oppositions of character. He was a non-combatant: there was too much kindness in his disposition to allow any fight to show itself. Impulsive and enthusiastic, with strong opinions and new views, he was never engaged in a scientific controversy. . . . William Rowan Hamilton's preservative was his dread of wounding the feelings of others. In his youth "Defender of the Absent" was his nickname . . . He had a morbid fear of being a plagiarist; and the letters which he wrote to those who had treated like subjects with himself sometimes contained curious and far-fetched misgivings about his own priority. But with all this there was a touch of the national temperament in him . . . an Irishman who never gets into a row may give quick but quiet symptoms of opposition of opinion, and of what, were it more than a rudiment, would be called pugnacity.

'Hamilton was apt to work by fits and starts. He has been known several times to work fourteen hours in one day, standing nearly all the while; but there were intervals of comparative inaction . . . Sometimes a letter was written and copied which was not sent for months, and then only the first sheet, with promise of the rest. It has even happened that the letter was knowingly

never forwarded at all, and that when, long after, he found reason to wish to send it, he could not find it and sent the copy instead. But with all this he made more notes than anyone, and was exceedingly particular about minute accuracy of points, crosses, and dates in the most trifling memoranda. His first *Lectures on Quaternions*, to our knowledge, had a dozen sheets printed off by December, 1851, and appeared only in the middle of 1853; the second set, which will probably appear by the time this account is printed, took a much longer time in passing through the press. The proof-sheets were held in hand until the author had satisfied himself about himself, and about others: and neither was easy work. . . . His papers were in most picturesque confusion, but he knew how to lay his hand on any one he wanted [not always]; he could detect the removal, were it only by a quarter of an inch, of any out of hundreds, and any such offence against the laws of his study would throw him into what our informant calls a "good, honest, thundering passion.". . .

'In the matter of right and wrong, Hamilton was very simple-minded. To say he was truthful would be only a part of the truth; his aptitude to entertain misgivings, already alluded to, made him often think it right to express his opinions to avoid the possibility of being misunderstood. But it may be said that it was not he and others who differed, but his opinions and the opinions of others; his tolerance was perfect. . . .

'He very much liked Goldsmith's writings, and we think points of similarity might be traced between him and the author whom he so much admired. But the parallel would break down altogether in one point; Hamilton spoke as well as he wrote. His voice was distinct, sweet, and powerful. He relished the extremes both of simplicity and splendour, though in his own habits and manners as plain as possible. He thought much of the comfort of others and lightly of his own. When some house-breakers were caught on the premises, and detained until they could be carried before a magistrate, he amused his family by directing that the felons should be asked whether they preferred tea or milk for breakfast. A full memoir of his private and public life would present a genial combination of intellectual greatness, moral goodness, and piquant peculiarity of thought and manner, all brightened by never-ceasing benevolence of feeling, and toned by rare gentleness of manner.'

In reference to the combination in Hamilton of poet and mathematician, De Morgan adds:—

'The moving power of mathematical *invention* is not reasoning, but imagination. We no longer apply the homely term *maker* in literal translation of *poet;* but discoverers of all kinds, whatever may be their lines, are *makers;* or, as we now say, have the creative genius.

'Hamilton was once called the *Irish Lagrange*, and the comparison was a good one. The styles of mathematicians differ as much as the styles of poets; and Hamilton is distinguished by that power over symbols, combined with elegance of expression, which is so remarkable in the writings of Lagrange.'

From the *Éloge Historique*, pronounced by Fourier on Laplace in the *Académie Royale des Sciences*, on the 15th of June, 1829, I extract the passage in which the great mathematician who delivered it describes the work and character of Lagrange. It will be found, I think, largely to justify the appellation above recorded as applied to Hamilton.

'Le trait distinctif de son genie consiste dans l'unité et la grandeur des vues. Il s'attachait en tout à une pensée simple, juste, et tres-élevée. Son principale ouvrage, la *Mécanique Analytique*, pourrait être nommée la Mécanique Philosophique, car il ramène toutes les lois de l'equilibre et du mouvement à un seul principe; et ce qui n'est pas moins admirable, il les soumet à une seule methode le calcul dont il est luimême l'inventeur. Toutes ses compositions mathématiques sont remarquables par une élégance singulière, par la symétrie des formes et la généralité des methodes, et, si l'on peut parler ainsi, par la perfection du style analytique. Lagrange n'etait pas moins philosophe que grand géométre. Il l'a prouvé, dans tout le cours de sa vie, par la modération de ses désirs, son attachement immuable aux intérêts généraux de l'humanité, par la noble simplicité de ses mœurs, et l'elévation du caractère, enfin par la justesse et la profondeur de ses travaux scientifiques.'

At the Stated General Meeting of the Royal Irish Academy,

held on the 30th of November, 1865, Hamilton's *Éloge* was pronounced by the President of the Academy, Dean Graves, now Bishop of Limerick. The *Éloge* is printed at full in the *Proceedings* of the Session, but I think it right to reproduce here passages from it which furnish interesting particulars not elsewhere dwelt upon as to the *modus operandi* of Hamilton in carrying on his scientific work, and convey on other important points testimony valuable and implicitly to be trusted, as coming from one who, having been long his scientific associate and private friend, was a specially competent witness:—

'Hamilton was gifted with a rare combination of those qualities which are essential instruments of discovery. He had that fine perception of analogy by which the investigator is guided in his passage from the known to the unknown. This is an instrument by which many important mathematical discoveries have been effected. Sometimes the mathematician devises some happy modification in the statement of a theorem or a method, by which its application may be extended. Sometimes, by analyzing different demonstrations, he even sees that a particular proposition may be made the starting-point from which he ascends to more than one generalization. In the investigations of Hamilton we find abundant instances of the skilful use of all the ordinary expedients and instruments of inventive sagacity. But he seems, also, to have possessed a higher power of divination—an intuitive perception that new truths lay in a particular direction, and that patient and systematic search, carried on within definite limits, must certainly be rewarded by the discovery of a path leading into regions hitherto unexplored. Something like this was the unshaken assurance which led Columbus to turn his back upon Europe, to launch upon the broad Atlantic, and seek a new world in the far-off west.

'And our illustrious countryman's diligence in research was not less admirable than his prescient sagacity. No amount of labour to be incurred could deter him from entering upon the calculations by which the correctness of his conjectures was to be tested. The confident expectation of obtaining results instructive in one way or another reconciled him to the irksomeness of the most tedious and complicated calculations. He felt that the great

object to be sought, in the first instance, was the discovery of the result itself; and he trusted that, once it was reached, he would be able to strike out some more direct and more elegant method of investigation. His MSS., even his published researches, furnish many examples of this. Once he had reached the conclusion at which he had been aiming, he resumed the consideration of the principal steps in his argument; he interpreted them with care; he traced their connexion, and seldom failed to arrive at simplifications and generalizations, which amply compensated for the labour spent upon his first essays. By this habit of grappling courageously with the difficulties of calculation he was distinguished from some other eminent mathematicians. Averse to plunge into depths of calculation from which they see no certain hope of emerging in the end, they are tempted to expend an undue amount of intellectual energy in the endeavour to force their way by a direct method to the desired result. . . .

'In the case of Hamilton, it is, moreover, deserving of notice that he evinced a readiness to grapple with the difficulties of calculation, even where there was no prospect of his labour being rewarded by any discovery. He engaged in exercises of this kind sometimes from a wish to strengthen his intellectual hold of general propositions by scrutinizing the results obtained by applying them in a number of particular instances; and sometimes, perhaps, from a wish to mature and keep in exercise those powers of calculation upon the exactitude and prompt operation of which so much depends in the conduct of difficult mathematical investigations. I have known him spend hours, or even days, in working numerical examples of some theorem in pure or applied mathematics, or in testing the accuracy of some formula of approximation. Occasionally he engaged in tasks of this nature, in the kindly endeavour to convince some half-crazed squarer of the circle that his proposed construction was inaccurate. Finding almost always that it was hopeless to convince the mathematical fanatic of the unsoundness of any of his premises, he would take pains to show him that the results he obtained were false in particular instances.

'And this leads me to notice a feature in his character which deserves to be recorded. From the lofty height of his genius and learning he was accustomed to stoop with the utmost readiness to hold converse with inferior minds. Many of his visitors at the

Observatory, and the members of the class who attended his lectures in Trinity College, can recall instances of his patience and good-nature in answering their questions, and clearing up the difficulties which beset them in their elementary studies of mathematics and natural philosophy.

'It is remarkable that while he possessed such powers of calculation, and was almost prodigal in the exercise of them, he was to the last degree solicitous about the metaphysics of every subject on which he undertook to write. We have seen a decisive instance of this tendency of his mind in his treatment of algebra considered as the science of pure time. So, again, in laying the foundation of his Calculus of Quaternions, we see him labouring to secure its stability by the most careful regard to the primary conceptions of time and space. Students of his *Lectures on Quaternions* have sometimes complained that he has claimed from them too much attention to the metaphysics of the subject, and has stopped them in their career of building up, in order that they might contemplate afresh the plan of the structure. But this was in accordance with his views regarding the ascending scale of the subjects of human thought. To religion he gave the highest place—and this not as a formality; for his was a deeply reverential spirit. He assigned the next to metaphysics. To them he subordinated mathematics and poetry, and assigned the lowest place to physics and general literature. His studies in the department of metaphysics were extensive. After a thoughtful examination of Berkeley's writings, he professed himself a disciple of that philosopher, "with most cordial and delightful submission"; not, indeed, assenting to every separate argument, but embracing his grand results;* and in this attachment to Berkeley's theory we have reason to know that he was confirmed by his converse with Faraday, who in his own region of investigation had been led to the conclusion that forces, rather than material particles, were the ultimate objects of physical inquiry. His acquaintance with the German language enabled him to master the works of Kant. In the reasonings of that philosopher, he was the more ready to concur, as his own previous inquiries had already conducted him to several of Kant's views respecting the intuitions of time and space. . . .

* See, however, *supra*, p. 177.

'His poetical compositions were the genuine outpourings of a noble heart and fervid imagination, characterized by a depth of thought and elevation of sentiment which compensated for occasional defects in artistic execution. These poetic efforts have an additional interest, as exemplifying in his own productions the connexion which he so strongly insisted on as existing between the highest provinces of science and the region of poetry—in both of which he maintained that there was scope and demand for the exercise of the imaginative faculty. According to him, the modern geometry, which deals with the infinites and imaginaries of space, has its beauty and its fascination; and he reckoned the happy daring of such geometers as Poncelet and Chasles as closely allied to poetry. We happen to know that this view of his, as communicated by him to the poet Wordsworth, was to the latter an entirely new revelation, and had the effect of raising his conception, which had before been unduly depreciatory, of the dignity both of science itself and of its most eminent votaries.'

The testimony which follows in the *Éloge* to the faculty for business manifested by Hamilton in his discharge of the office of President of the Royal Irish Academy has been quoted in the second volume of this work, page 245, and need not be here repeated. Dean Graves then proceeds:—

'... A mathematician endowed with such original powers as Hamilton possessed might have been excused, if, yielding to the natural temptation of waiting for casual inspirations, he had carried on his labours in a desultory or unsystematic manner. To such temptations—and no doubt he felt them—he rose superior. He was, on the contrary, remarkable for the diligence and method with which he performed all his work. These qualities are evidenced by the number, magnitude, and importance of his published works. There was no minute care, even in matters of typographical nicety, which he disdained to expend upon them. And in his MS. books, carefully written, and with dates marking from day to day the progress of his scientific life, he recorded all his meditations, all the calculations through which he passed in his apparently fruitless, as well as in his most successful, researches. These volumes, many of them very large, and numbering about sixty,

have been deposited in the Library of Trinity College. They will supply to future historians of science the most precious materials illustrating the development of Hamilton's discoveries. They will exhibit, doubtless, germs of thought suggestive to others of new discoveries. They record a great *commercium epistolicum*—his correspondence with the most distinguished scientific men of his own age. Nay, more, they will be found to contain memoirs on a variety of subjects, complete in themselves, and carefully elaborated, but which he had abstained from publishing, either because they were unconnected with the greater works which he had in hand, or because he hoped to develop them more fully at some future time. It is to be hoped that they will yet see the light, and like the posthumous memoirs of Euler, inspire us with a feeling that their great author is still holding converse with us. It will be a satisfaction to the members of this Academy to be told that his *Elements of Quaternions*—the work upon which he was engaged with most unceasing activity for the last two years—is all but complete. I have reason to know that at no period of his life—not even when he was in the prime of health and youthful vigour—did he apply himself to his mathematical labours with more devoted diligence. Those who did not actually know how he was employed, or who had formed a false estimate of his character, might imagine him indolently reposing upon his laurels, or pursuing his studies in a desultory way. Such a conception of them would be the very opposite to the true one. His diligence of late was even excessive—interfering with his sleep, his meals, his exercise, his social enjoyments. It was, I believe, fatally injurious to his health.'

I will not refrain from adding the prognostication with which the *Éloge* concludes:—

'Believe me, gentlemen, the fame of Sir William Rowan Hamilton, great as it was during his lifetime, will become yet greater when the world has been furnished with materials enabling it more perfectly to estimate the variety and richness of his endowments and the value of the services which he has rendered to science. His reputation, even now, does not rest on the partiality of friends and countrymen. The learned men of all lands have already declared him worthy of the highest honours which can be paid to intellectual eminence. This world-wide recognition, at the present

time, of his genius and discoveries, affords us a sure pledge and earnest of the perpetuity of his reputation, and warrants us in regarding his name as a glory which is not to pass away from the scientific and literary chaplet of Ireland. And in this fact and this anticipation we may thankfully and happily behold a full justification of his own early, and it might have been feared enthusiastic, aspirations—of his deep and generous consciousness that he was intrusted with faculties and powers capable of achieving in the noblest fields of thought a worthy fame both for himself and for his country.'

It will be interesting here to recall, in connexion with the reference made by Dean Graves to the early aspirations of Hamilton, a passage in a journal written by him when a youth of twenty, in which he deliberately renews for himself words of aspiration to which he had given expression at a still earlier time. Recording the effect produced upon him by the contemplation of a great work like the *Principia* or the *Mécanique Céleste*, he goes on to say, in words combining modesty and strength:—

'When I see how much others have done, and contrast with it the little to which I have attained, the effect is painful but salutary. It seems practically to impress that eminence cannot be attained without exertion; it teaches modesty of the most genuine kind, and in the most natural manner; at the same time it acts as a powerful stimulus, and kindles the ardour of my aspiring after that fame which (as I once expressed it in a letter to my uncle) is the "meed which Genius and Industry when united have sometimes been so fortunate as to obtain, with the world for their arena, and all time for the tribunal; which has wedded to immortality some favoured names, and marked out some individuals as the instructors of mankind."' (Vol. i. pp. 206–7.)

A few words in a letter from Sir John Herschel to Lord Dunraven seem to me with great power to express a truth of high value in reference to Hamilton's enthusiasm. He writes:—

From Sir John F. W. Herschel *to the* Earl of Dunraven.

'Collingwood, *December* 19, 1865.

'My dear Lord Dunraven—A great many thanks to you for sending me Dean Graves's *Éloge* on Sir William R. Hamilton, which is admirable!—in perfect good taste, and not a word more than was fully deserved.

'He *was* indeed a most admirable person, and a most truly amiable and high-souled one. Nothing but so much greatness could have made so much enthusiasm only what was natural—and nothing but so much enthusiasm could have carried him on to so much greatness.'

An *Éloge* of high tone and ability was also pronounced on Hamilton, in the year 1866, by the Rev. Charles Pritchard, at that time President of the Royal Astronomical Society; and to the September number in the same year of the *North British Review*, Professor Tait contributed a lucid and powerful exposition of Hamilton's scientific achievements, interesting, from its style and substance, alike to the general reader and the student of science, and worthy of a disciple whom Hamilton in one of his latest notes to the *Elements of Quaternions* (p. 755, note) designated as 'eminently fitted to carry on, happily and usefully, this new branch of mathematical science, and likely to become in it . . . one of the chief successors to its inventor.'

From these testimonies of eminent Men of Science I turn for the last word to the Poet Friend, known to Hamilton for five-and-thirty years, and at the end of fifteen additional years uttering in verse an affecting proof that the feeling of admiring and affectionate friendship was in his heart as deep and fresh as ever.

I have already given to the reader one of two sonnets written by Aubrey De Vere, in 1880, in memory of Hamilton.* Addressed

* *Supra*, p. 159.

to the spirit of his friend, it recalls attributes of his character which may here fitly be reproduced:—

> 'Thy soul translucent, yet thine insight keen,
> Thy heart's deep yearnings and perpetual youth;
> Thy courtesy, thy reverence, and thy truth—.'

The sonnet which I reserved is the following retrospect of their friendship; it sets before us, in succession, the two congenial companions united in the noblest aspirations; the survivor, filled with 'beaming memories'; the departed, at the last in full secure possession of the truth he loved. Who can repress the hope that they will be re-united?—

> 'Friend of past years, the holy and the blest,
> When all my day shone out, a long sunrise;
> When aspirations seemed but sympathies,
> In such familiar nearness were they dressed;
> When song, with swan-like plumes and starry crest,
> O'er-circled earth, and beat against the skies,
> And fearless Science raised her reverent eyes
> From heaven to heaven, that each its God confessed
> With homage ever widening! Friend beloved!
> From me those days are passed; yet still, oh, still,
> This night my heart with influx strange they fill
> Of beaming memories from my vanished youth:
> On thee—the temporal veil by Death removed—
> Rests the great Vision of Eternal Truth!
>
> '*January* 10, 1880.'

CHAPTER LII.

(SUPPLEMENTARY.)

A GATHERING OF FRAGMENTS.

In looking back over the letters and other documents in my hands, I have come to the conclusion that some items, either accidentally overlooked or that found no natural place in the biography, have been omitted, which nevertheless are worthy of preservation as either expressing a thought of value or casting additional light upon some feature of Hamilton's character.

Under the head last mentioned I give a passage from a fragment of a letter to Aubrey De Vere which is undated, but which must have been written in September, 1842 (see *supra*, vol. ii. pp. 393–4), soon after his perusal of Henry Taylor's *Edwin the Fair*:—

'My copy of *Edwin* has been carried off by one of my nieces at Dunsinea, and I shall only at this moment say that I was particularly tickled by the sketch of Wulfstan the Wise, which, if I enjoyed the acquaintance of the author, I might have fancied to be a hit at me. The two volumes of Tennyson have only this instant reached me, and I have not opened them yet.'

The sketch referred to is in *Edwin the Fair*, act ii. scene 2, where Earl Leolf thus describes Wulfstan:—

> 'This life, and all that it contains, to him
> Is but a tissue of illuminous dreams
> Fill'd with book-wisdom, pictured thought, and love
> That on its own creations spends itself.
> All things he understands and nothing does.

Profusely eloquent in copious praise
Of action, he will talk to you as one
Whose wisdom lay in dealings and transactions;
Yet so much action as might tie his shoe
Cannot his will command; himself alone
By his own wisdom not a jot the gainer.
Of silence and the hundred thousand things
'Tis better not to mention, he will speak,
And still most wisely.' *

There was doubtless some ground for applying such a description to himself, however he may have laughingly felt that in such application it was a caricature, and we may remember his noting in early life (vol. i. p. 620) that he was conscious of having, like Coleridge, too much of the element of πάθος in his mental constitution; but it is also true that, when there was an adequate call upon him for the exertion, he could display much practical ability. I remember Dr. Humphrey Lloyd being struck by this after perusal of some early letters to Lord Adare, giving directions for the setting up of a dial—letters, however, which were too elementary to print—and I have already adduced testimony, which has been confirmed to me from many quarters, of his power promptly to apply in the way of practical decision his thorough knowledge of the constitution and laws of the Royal Irish Academy when acting as its President. A more adequate estimate of his character, drawn by his own hand, may be deduced from the following letter to Mrs. Wilde, whose poetic temperament

* The continuation of the letter is worth giving in a note. Amusing in itself, it furnishes an additional instance of Hamilton's observing interest in the intellectual progress of his boys. '. . . My eldest boy is not quite so well as usual to-day . . . I was much amused at the development of the opinions of his younger brother [Archibald Henry] a few mornings ago on the voluntary principle, which, when he found incapable of being pushed into a rigorous universality, he said that he should like to *keep for himself*, and to let the *in*voluntary principle apply to other people. But William Edwin argued that if the voluntary principle were to be carried fully out, each man might complain of God for not giving him everything he wished to have—more money, more rich fruits, and a grander and more beautiful earth.'

sometimes prompted her to give enthusiastic expression to her admiration of Hamilton's intellectual powers.

'Observatory, *February* 11, 1858.

'You know that whatever it may be at any time your pleasure to say, even it if be in praise of myself, I submit to it, from the profound conviction felt by me of your being an entirely truthful person.

'Of course it is needless to say that I am not to be considered as *adopting* any expression which it would not have been modest enough in *me* to have first uttered, but to which I have (once or twice) listened, without remonstrance, from you—for the reason mentioned above.

'It may not sound very consistent with any such professed humility on my part, if I say to you that, after having served for the Quaternions during fourteen years, and having (as America seems to think) won my Rachel—to be my own by an intellectual marriage—I now wish to wind up several scientific projects, from which those quaternions had for a long time diverted me; and feel as if I were entering, or had already entered, on a new harvest of labour and reputation.—As to *Fame*, if it have not been won or earned already, it is not likely that any future exertion will make it mine.

'But as to the Labour; *that* is a thing within everybody's power to judge of, even for himself. I have very long admired Ptolemy's description of his great astronomical Master, Hipparchus, as ἄνηρ φιλόπονος καὶ φιλαλήθης: "a labour-loving and truth-loving man."—Be such my epitaph!

'Since I have presumed to translate (unnecessarily for *you*) a bit of Greek prose, let me try my hand at a small portion of Latin verse, of which I am reminded by the subject of this note. It occurs almost at the end of Juvenal's celebrated Tenth Satire: Johnson's very clever imitation of which ("Vanity of Human Wishes") is not within my reach just at present. The lines to which I refer stand thus in the original from which I copy: or at least in a book given me long ago by Dr. Brinkley, who knew that I enjoyed the Classics, and which is dated, "Parisiis, 1528," and is printed throughout in the *Italic* character:—

'"Ut tamen et poscas aliquid, voveasque sacellis
Exta, et candiduli divina tomacula porci,
Orandum est, ut sit mens sana in corpore sano.
Fortem posce animum, et mortis terrore carentem,
Qui spatium extremum vitæ inter munera ponat
Naturæ, qui ferre queat quoscunque labores,
Nesciat irasci, cupiat nihil, et potiores
Herculis ærumnas credat, sævosque labores,
Et venere et cœnis et plumis Sardanapali."

'My translation of a few minutes ago is as follows:—

'"Yet if thou must ask something of the gods,
Pray that there be sound mind in a sound body.
Ask a firm soul, one void of fear of death;
Which may rank life's last space with boons of Nature;
Be able to endure whatever labours;
Be never angry; covet nothing; count
Better the pains and the stern tasks of Hercules,
Than pleasures, feasts, and down of Sardanapalus."'

To the word 'carentem,' in his transcript of the original passage, the writer adds the marginal note, 'For "carentem" a Christian would have written "liberatum."'

It may be felt that in the two quotations here given, the Greek and the Latin, Hamilton recognised the ideal of a character strong and lofty, the possession of which he was conscious of having habitually aimed at, and of having in great measure attained.

As another contribution to the estimate of his character, coming from his own hand, I add a short extract from a note to an intimate friend, written at the close of 1852:—

'. . . Indeed I have much for which to seek forgiveness both from God and man—but certainly I have never caused pain for any gratification of my own. . . .'

Hamilton's tolerance of opposing opinions has been noted by Professor De Morgan (*supra*, p. 218); it was a tolerance not of mere good-nature or indifference. The following extract from a letter to Mrs. Wilde shows that in the subjects by which tolerance is most severely tested, religion and politics, he wisely recognised

the true ground for tolerance, namely, the large extent over which persons of good sense and good feeling were at one, however they might differ in religious creed or political party:—

'In religion and politics men can't, they ought not to think one view as good as another. We must not say, like Tooks in Dombey, it is "of no consequence at all": but each may give his neighbour credit for being as sincere as himself. I think, however, that with most people who do not vigorously shut their hearts against receiving impressions from others, and who mingle at all with the world, or even read pretty freely, there grows up gradually a feeling that for the most *essential* purposes of life, including thoughts and conduct, people who differ can yet sympathise with one another.'

The sonnet which I here introduce would have been inserted after Hamilton's letter of January 30, 1835, to Aubrey De Vere, had it not been accidentally overlooked by me. It was written at the conclusion of some weeks spent at the Observatory in solitude, and on the point of his starting to pay a short visit to his wife and child, and Mrs. Bayly, at Bayly Farm, near Nenagh (see vol. ii. pp. 118, 120). Mrs. Bayly was the 'Mother' of the last line.

'Draws to its close a melancholy while,
A long long night of absence; from the sky
Melts off the solid gloom; pale phantoms fly,
And soon the blushing dawn will brightly smile;
And like a man who many a weary mile
Hath travelled lonely, if at length his eye
Discern the wish'd for fountain, so feel I,
A traveller near the Sources of the Nile.
What I have pined for, what has been a power
Guiding my steps through time as his through space,
Imagined though unseen, will face to face
Repay me soon for many a lonely hour:
When I shall clasp, in a remember'd bower,
Mother and wife and child in long embrace.

'OBSERVATORY, *January* 31, 1835.'

This sonnet is a strong proof of the affectionate feeling which Hamilton never ceased to cherish towards his wife. Other

instances have been given. Yet it cannot be denied that the whole course of their married life proved the justness of her early foreboding, arising from a sense of weak health, both of body and mind, that she was not fitted to sustain the burden of duties properly devolving upon a wife in her position. The following letter I impart to the reader, principally because, written when he had been more than twenty years her husband, it manifests the continued warmth of his conjugal affection, and the considerate thoughtfulness of a true paterfamilias, and, at the same time, shows how he could even to herself playfully allude to her extreme shyness and retiredness. I remember hearing Dr. Lloyd say that he had been often at the Observatory, but had never seen Lady Hamilton.

'LIVERPOOL, *September* 27, 1854.

'MY DEAREST HELEN—The Association has just been adjourned to Glasgow, to which place I had moved, in the General Committee, that we should go next year. They got me up to make another speech, just now, proposing thanks to the foreigners, and I came out with flaming allusions to the war and our French allies. I mentioned my having learned mathematics chiefly from French books, but said that the authors of them, as I had never visited Paris, had appeared to me almost as abstract ideas. (You know that Lady Hamilton was once called by Dr. Robinson "an abstract idea.") But I amused the audience by turning to the fat little Abbé Moigno, of whom Will has heard me speak in connexion with Cauchy, and who was sitting near me, and by saying, "For instance I had thought the Abbé here to be an abstract idea: but I think that you will all agree with me in considering him to be a very concrete and a very pleasant body"! Arch promises you that he will not become a "damp unpleasant body," if he can help it. But he ought to have written to you ere this. Indeed I know that he *did begin* a letter on the day of our arrival here, but mislaid it among my papers. He seems to have enjoyed himself greatly. I am quite tired after all the meetings I have attended to-day, and yet hope to attend another to-night, after resting a little at my lodgings. I cannot write to-day to anyone but you, but please to tell dear Moo [pet-name of his daughter] that I

received her letter, and that our lessons in French have been of the greatest use to me. Give love to William also, and believe me to remain your very affectionate husband, W. R. Hamilton. Please write to me immediately, or let Will or Moo do so, directing as before. Arch and I will probably start for the Lakes on Saturday.'

Hamilton's interest in children was not confined to his own. It was characteristic of him that he treated all children not only with tenderness but with respect. An extreme instance of this has been communicated to me by his surviving sister, from whose letter I transcribe it, rendered specially interesting, as it is, by the warmth of feeling with which it is narrated:—

'Before leaving Dublin for the neighbourhood of Blackrock I went out to spend a parting day at the Observatory, bringing with me a dear little charge, a sweet little girl of weak powers. Her mind had never opened *at all*, till she was put under my care. She had been in the Earlswood Institution for Idiots, but was no better, rather worse after a year. When she came to me she could not speak so as to be understood, could not hold a spoon or needle, and so on. This will just give you an idea of the little girl. It pleased God that I should see that she was not idiotic. She had memory, affection, the power of distinguishing colours and people, pity and sympathy, manifested when I showed her a picture in which were represented dead camels in the Desert, &c.; and she won upon my heart, and she loved me beyond anything on earth, just because I first showed that I considered her worth trouble. "Lizzie CAN *taught*" was her delighted expression, when she succeeded in buttoning her cuff for herself. She grew up a lovely girl, and Ellen, another young person with much talent and *no* deficiency, whom I trained from a child, and who is with me here, taught my poor Lizzie to sing sweetly, and she grew on improving miraculously till she was thirteen, when she died and went to Jesus, whom she loved. Still she was, as you can well suppose, far, far behind even common *intellects* in many respects. Well, I brought her to the Observatory: my dear brother and [his daughter] Helen knew her well at my lodging in Dublin. I left her in the drawing-room and went up with Helen to her room, and we stayed there talking, I knowing well that Lizzie would remain quietly

below till I went down: but after a time a message came up to me that Sir William sent to tell me "that he was now obliged to go to his library and Miss Lizzie would be alone;" he could not bear to be unpolite even to *her*. Of course I went down at once to ease *his* feelings, not the dear child's. He told me that he had played a game on the Icosian board with her, and "though of course I saw that she did not understand it," said he, "yet I assure you that many a duchess could not have carried it off so well. She had to me quite the air of one accustomed to Castle society, who considered that she *condescended* to oblige me." I almost felt tears rise as I listened to the truthful humble words of that Giant in Intellect, and thought of the pitiful sneers that poor nobodies would give at the idea of sitting down to play any game with poor Lizzie. He sent us into town on the car after dark, and he said to me, "I think her too interesting; I hope she will not attract anybody too deeply to her; but I should fear it for her." His humility was to me the most wonderful thing about him.'

She adds, as an anecdote illustrative of his truthfulness:—

'He gave a question to the boys of Lovell Edgeworth's school which they could not answer. Presently he found that the solution was impossible, and he at once avowed his mistake, for which prompt confession the boys gave him a tremendous cheer.'

There must have been something frank and engaging in his way of doing what most examiners in a similar case would have done; for I find the incident (which occurred in 1828) referred to in a letter of Maria Edgeworth.

It will not be wondered at that Hamilton's kind feeling extended to our dumb fellow-creatures. In a letter written when very young, from Drumcondra, he speaks of his spending the earliest hours of a summer morning in reading Shakespeare and playing with two kittens whom he had taken to bed with him, and his sister writes—

'He was always fond of cats, and might often be seen writing some mathematical paper with a kitten or favourite cat on his shoulder playfully trying to catch the pen.' 'His politeness was almost a

rebuke to others. A young lady, who was living with me in Dublin at one time said, "I never saw so polite a gentleman as your brother; I think he would almost bow to a cat;" and I was reminded of her and amused him by repeating this to him one day, when accidentally he did tread upon the cat's paw, and turned round, and smiling said, "I was going to say, I beg your pardon."'

His feeling consideration for all living things around him drew towards him from them unbounded confidence. One instance of this made a deep impression on those who witnessed it, and indeed it was an occurrence fitted to excite and to excuse a somewhat superstitious wonder. On a Whitsunday morning, as he was reading prayers in the centre of his assembled household, a dove flew in through the open window and settled on his head; it was undisturbed by Hamilton, who continued to read, and after an interval it peacefully flew out.

It is not on record, I believe, that he ever killed or even struck in anger his mute fellow-creatures. A single exception, not uncharacteristic, in regard to the latter statement, is related by his son as told by himself. Finding the greyhound, Smoke, one day tearing a Book of Common Prayer in the library, he thought it his duty, at some personal risk, to inflict upon him a serious chastisement. 'This,' Mr. W. E. Hamilton says, 'was in his High Church days.' That he possessed abundant physical courage, and a strong sense of personal dignity, was habitually manifested by him: one illustration is the fact, that in his earlier days he challenged to a duel a member of the Royal Irish Academy, who, as he conceived, had impugned his honour or truth. His friend, Colonel Larcom, whom he engaged as his second, succeeded in obtaining for him adequate verbal satisfaction.

On the authority of Hamilton's intimate friend, Dr. Samuel O'Sullivan, the following report of an interchange of compliments between Wordsworth and Hamilton has been communicated to me: the circumstance that only an astronomer could have imagined such a reply seems to authenticate the anecdote:—

'When Wordsworth visited Hamilton at the Observatory [in 1829] he took occasion to say, "I feel happy in a pleasure rarely enjoyed by me, that of being in the company of a man to whom I can look up." "If I," replied Hamilton, "am to look down on you, it is only as Lord Rosse looks down in his telescope to see the stars of heaven reflected." '

A reply of Hamilton's, more in his style, calls for record. When asked whether he accepted, as expressing a truth, Locke's comparison of the state of the human mind at birth to a sheet of white paper, he said, 'Yes, but *ruled* paper': an answer pregnant with much of his philosophy; which in outline admits perhaps of no nobler adumbration than it has received from Wordsworth in the lines which form part of what he calls the *Prospectus* of his poem, 'The Recluse':—

> 'while my voice proclaims
> How exquisitely the individual Mind
> (And the progressive powers perhaps no less
> Of the whole species) to the external World
> Is fitted: and how exquisitely, too,
> Theme this but little heard of among Men,
> The external World is fitted to the Mind;
> And the creation (by no lower name
> Can it be called) which they with blended might
> Accomplish.'

It will be remembered how emphatically Hamilton distinguished in the mind the faculty of Intellect from the faculty of Faith. He felt, if I recall rightly what I heard from him, that no small part of the proof of the existence of God rested upon the fact that the great Idea fills, as no other idea can fill, the aspiration of Faith. In one of his Manuscript Books (I. 1864, pp. 1512) Hamilton transcribes from pencilled notes of 1852, one upon Pantheism. Confessing that Coleridge's statement does not produce in me the satisfaction it appears to have given to Hamilton, I produce the note principally on account of the comparison, in regard to mathematics applied to metaphysical reasoning, of Coleridge and Kant.*

* See vol. ii., pp. 137, 142.

'Pantheism, much more than Popery, appears to be the danger of our age. Coleridge confessed to me, and regretted—the confession indeed was needless, but the regret was interesting—that he had never studied mathematics. Accordingly, in his philosophical writings, as a general rule, his mathematical illustrations have seemed to me to obscure the subject (while those of Kant throw light upon every point to which he applied mathematics). But I must make one signal exception, on this very subject of Pantheism, Coleridge's mathematical illustration of *it* appearing to me to be *perfect*.

Let G (said he) stand for God,
and W for the World:

then theist and pantheist *agree* in asserting the formula,

$$W - G = 0;$$

"The World, without God, is nothing." But, continued Coleridge, the pantheist inverts the formula, and says also,

$$G - W = 0:$$

or, "God, without the World, is nothing." Whereas (he said) the theist, on the contrary, asserts that

$$G - W = G:$$

or that "God, *without* the *manifestation* of himself, which he has been pleased to make in his created Universe, would still have been the *same personal God*."'

Turning now to Hamilton's own subject of Quaternions I transcribe a page from another of his Manuscript-Books (M. 1848. p. 73) which displays what was with him a favourite stanza of Beattie's *Minstrel*, and adds a comment. One can imagine how pleased was the inventor of Quaternions with its opening lines:—

From BEATTIE'S *Minstrel.*

'And Reason now through Number, Time, and Space,
Darts the keen lustre of her serious eye;
And learns from facts compared the laws to trace,
Whose long progression leads to Deity.
Can mortal strength presume to soar so high!
Can mortal sight, so oft bedimmed with tears,
Such glory bear!—for lo, the shadows fly
From Nature's face; confusion disappears,
And Order charms the eyes, and harmony the ears.'

'I had copied the foregoing extract from Beattie into another book, long before I thought of the Quaternions; but it afterwards occurred to me that there was at least some distant analogy between the view of science taken in this passage, and that which I sought to embody in the two following lines of my own Sonnet entitled the *Tetractys*, which was one of the two that were jointly called by me "Recollections of Collingwood." *

> "And how the One of Time, of Space the Three,
> Might in the chain of Symbol girdled be."'

Early in 1866 Mr. W. E. Hamilton furnished me with some memoranda made by him respecting his father's habits of work and traits of character, the substance of which I here reproduce as the truthful notes of an observer possessed of special advantages.

It will be remembered by the reader of this biography that Hamilton was accustomed, even up to the last year of his life, to work continuously in mathematical research or arithmetical calculation for very many consecutive hours, the processes entered upon often requiring for completion such prolonged labour. To stop in the middle and *rest* he found impossible, for the mind would still work on, whether he laid down the pen or not, and he feared to lose the thread of argument or investigation. To continue to the end a task, in which good progress had been made, required, as he was convinced, support and stimulus for the brain, and this he administered to himself in the injurious form of porter taken in small sips as he felt fatigued. The need thus experienced, connected as it was, with his disinclination to be disturbed at his work by regular meals, was, according to his son's testimony, the principal cause of his recourse to alcoholic stimulant, for which he admits that his father had besides a constitutional proclivity, as well as a disposition, arising from his genial nature, to conform to the prevailing custom of the time when he first entered into social life.

Mr. Hamilton notes that his father was fond of teaching,

* See vol. ii., p. 525.

partly from a genuine desire to impart knowledge to those whom he happened to address, who, however, were often incapable of receiving it, but not less from a felt necessity of 'throwing his mind into a didactic attitude,' in order that he might put the result of laborious analysis into the best form for communication. I transcribe here Mr. Hamilton's graphic description of the processes he observed:—

'It was his habit invariably before making a communication to the Royal Irish Academy to lecture an audience, fit, I hope, but few, consisting generally of myself and Thompson, the assistant, although the ladies were sometimes present. This was not a mere rehearsal, for the actual wording in the R. I. A. might be very different, but it was the throwing of his own mind into the didactic attitude, and the satisfying himself that he had done so. In fact he generally passed through three stages: 1. That of mathematical investigation; 2. That of final polishing; 3. That of throwing his own mind into the didactic attitude. I generally knew when 3 was coming, and anticipated it by bringing the black-board, taking care to have plenty of chalk ready. Well, then, he generally began by a few preliminary scribblings on the board, speaking to himself and rubbing out the chalk marks. Then followed a lecture, in which it was very curious to watch his tendency to digress. He would say, with a sudden start, "Yes—wait—stop, I see another way of proving this; let ϕ log tan θ = &c., . . . —but, however, this will keep—to resume, x = &c. Well, now we have killed off the first part of the subject." So that in "lecturing" he suppressed the digressive tendency, but in conversation he yielded to it, and used often to say, "Well, what was I speaking of just before?"—"Of, &c. &c."—Sir W., "no—go still further back." It was very amusing to watch Thompson—whose ideas moved slowly, and who could only go a very small way in the subject—standing by, spectacled and owlish, and chiming in with an occasional "I see." Sometimes, however, my father threw a Parthian dart at him, such as "Just recapitulate the last six equations"; when it generally happened that Spica Virginis or α Lyræ required immediate attention. I will add that the effort to throw his mind into the didactic attitude did not always succeed. Sometimes he had got to a certain stage in the lecture when some

new track of discovery suggested itself, and he made a pause—noted sufficient to recover it afterwards—and went on with the lecture: so that the didactic stage required a conscious effort in suppressing the inventive tendency for the time.'

Under the head 'Procrastination,' Mr. Hamilton mentions of his father that 'he was almost invariably late for church, dinners, and public meetings of all kinds.' I think that Procrastination is not quite the right term for this short-coming. It arose, in my opinion, not from a weak habit of postponing what he had determined to do, but rather from a mistaken estimate of what might be accomplished by him in the interval preceding an appointment. He lost count of time when absorbed in his own work, and, in consequence, engagements of inferior interest were deferred; some, as it were, pushed forward so as to entail merely the lateness spoken of by his son; some pushed off the line into an undefined future. It was thus miscalculation of time as an element to be applied to practical uses, not a failure of intention or even of will. He came late to church or dinner, but still he arrived. He answered a letter a month, or a year, after he received it, but he answered it; sometimes, indeed, omitting to post his answer: and as, speaking broadly, he forgot nothing, he only wanted a stretching out of the twenty-four hours, or a boundless extension of life, to fulfil all his obligations. He was certainly open to the taunt, friendly or hostile, that, however great a master he was of 'pure time,' he was no adept in the management of sublunary time—of the time we have to deal with in this practical world of ours. And yet, after all, it may be asked, Is not the deficiency thus commented on the almost inseparable shortcoming as to minor activities of a great mind habitually employed in doing great things?* But the reader must not be deprived of his son's dramatic exemplification of work thus miscalculated with respect to time. This instance is, indeed, but a miniature of Hamilton's repeatedly unfulfilled prognostications of the completion and publication of his books.

* See as to Wordsworth, vol. i., p. 585.

'Mrs. Comerford could give instances of procrastination. She was much at the Observatory, and often used to go into town for marketing. One of Sir William's Laws of the Medes and Persians was "No servant, workman, postman, baker, College tradesman, or other person, shall go into Dublin or leave the Observatory without giving previous notice to me." Another, "Any person wanting any large sum (*e.g.* £5) must give ample notice (say a fortnight's) beforehand: anyone going into town for marketing and requiring any small sums shall give notice on the previous day." Mrs. C. sometimes forgot these rules, and one day was just going off at, say, 10 A.M. Dialogue: Mrs. C. "I am just going into town, uncle, and I want seven shillings to get tea, &c. Have you anything for town?"—Sir W. "Oh, *why* didn't you tell me last night? I could have had a package ready: you can come up in about an hour."—11 A.M. Mrs. C. "Well, uncle, are you ready? It's 11 o'clock; I wish you would let me off."—Sir W. "I think I'll wait to write some letters."—Mrs. C. "Shall I send the car round."—Sir W. "Certainly not."—Mrs. C. "The mare will be drenched."—Sir W. "You can come up in about half an hour."—11.30 A.M., Mrs. C., knocking. "Well, uncle!"—Sir W., with a groan. "*What* is it?"—Mrs. C. "It's teeming rain, the mare had better go round."—Sir W. (second groan). "Very well, I will wait for the evening postman" (4 P.M.).—4 P.M., postman comes. Mrs. C. "Well, uncle, I'll be late for shopping."—Sir W. (cheerfully and abstractedly to himself). "Well, at last I've conquered that."—Sir W., to me. "I've got the second elimination in a very compact form. Now, you see θ," &c.—Mrs. C. "Well uncle," &c.—Sir W. "Wait a moment." "Well then (to me) I see a very simple way—stop, don't speak for a moment."—Exit Mrs. C., whispering to me, "Get uncle to let me go or the shops will be shut, and our candles are out."—6 P.M., Mrs. C. (almost crying), "Uncle, the shops will be closed if you don't let me off."—Sir W. "μ^{-1} L = Well, I'll not mind writing any letters. I'll not keep you."—Mrs. C. "But you did not give me the seven shillings."—Sir W. "You should have told me before. However, mind I'm not keeping you."—Exit Mrs. C., in despair.'

Mr. Hamilton does not report much in connexion with religion, but states that his father did not hold the verbal inspiration of

Scripture, and that he used to express a belief in the semi-inspiration of Milton: that, attaching high importance to the doctrine of the Resurrection of the Body, he believed it to be a necessary but sufficient condition that the risen body should contain identically some material particle or particles of the old body, however entering into new chemical or organic combinations. He did not hold with the revivalists that it was the duty of laymen to try to convert others, but he considered it every Christian's duty, when infidel opinions were delivered in his hearing, to assert his own belief; a duty at least on one occasion publicly fulfilled by himself.

I may conclude this Chapter of Fragments by recording what I had the pleasure of hearing this summer (1887) from Professor Sylvester: that he had lately found, in Hamilton's Memoirs connected with Mr. Jerrard's Researches as to the solution of equations of the fifth degree, a discovery of Hamilton which had unaccountably failed to attract from succeeding mathematicians the attention it deserved. Professor Sylvester has made further development of this discovery, which he has communicated to *Crelle's Journal*, and to the *Transactions* of the Royal Society, in a memoir entitled *On Hamilton's Numbers*, and he mentioned to me as a remarkable fact, that the first step in this direction was made by Bring in the year 1786; the next, and most important, by Hamilton, fifty years after, in 1836; and the extension by himself at the end of another fifty years, in 1886. This is an example of a published result which, having been long neglected, has been at last recognised as valuable, and turned to account. I cannot but hope that among Hamilton's unpublished manuscripts may be found other mathematical results worthy of being given to the public, and likely to exercise profitably the attention of mathematicians.

In the ensuing pages the reader is presented with a copious selection from the very active correspondence which commenced in 1841 and was carried on, though with long intermissions, up to the last year of Sir W. R. Hamilton's life, between him and Professor De Morgan. It will be found to be of interest not only to the scientific but also to the general reader. Had it been exclusively mathematical, I should have left it to take its place in his purely scientific correspondence, of which there are large remains. Its title to be thus connected with the biography arises from the fact that in it, upon subjects outside mathematics, the wit of De Morgan and the geniality of Hamilton, the mutual confidence, the comprehensive sympathies, and the honest divergencies of the two friends found free and characteristic expression; while, at the same time, so often does the scientific element suggest the non-scientific, that it would be impossible without injury to both to part one from the other.

When about to transmit to me the correspondence, Professor De Morgan, in terms which demand my grateful acknowledgment, wrote to me as follows:—

'91, Adelaide Road, N. W., *July* 17, 1866.

'I have a letter from young Hamilton, who is off to the New World before I could answer, asking me to transmit to you Rowan Hamilton's letters to and from me for selection. This I shall be very glad to do; but you will find them an unsorted mass. If, however, you are going to set to work at a biography, you will find, as I have done, that a man must sort for himself, let who will try to precede him. Secondly, with reference to his request that I would pencil anything I want to be omitted, I shall do no such thing. I will trust to your general intention not to insert anything that would give pain to the living or offence to the nearest of kin to the dead. I think it unlikely that I should strike anything out of my own, unless something should occur which there could be no doubt about, and this you would be sure to settle without me.'

In making the following selection it has been my study to observe the rule here laid down.

A few of the earlier letters have been already inserted in the second volume of this work, but it has been thought expedient to reprint them in their proper places in the correspondence.

CORRESPONDENCE

BETWEEN

SIR WILLIAM ROWAN HAMILTON AND PROFESSOR AUGUSTUS DE MORGAN.

From PROFESSOR DE MORGAN *to* SIR W. R. HAMILTON.

'69, GOWER-STREET [LONDON], *May* 8, 1841.

'MY DEAR SIR WILLIAM—I hardly know whether you remember that we made a little personal acquaintance, some twelve years ago, when you were in London.

'I take this opportunity of leaving my card with you in the accompanying form* by the post.

'I shall be very glad to see the *Theory of Triplets* pointed at in your Paper on Algebra; time-triplets or space-triplets, I don't care which.

'In the meantime, I remain yours very faithfully,

'A. DE MORGAN.'

From SIR W. R. HAMILTON *to* PROFESSOR DE MORGAN.

'OBSERVATORY OF TRINITY COLLEGE, DUBLIN,
'*May* 12, 1841.

'MY DEAR SIR—I have within these few minutes received your Paper *On the Foundation of Algebra*, and have hastily cast

* Professor De Morgan's first Paper *On the Foundation of Algebra*, published in vol. VII., part ii., of the *Cambridge Philosophical Transactions*. See Preface to *Lectures on Quaternions*, p. (41).

my eye over it, intending to read and think about it afterwards. The handsome manner in which you have, there and elsewhere, expressed yourself respecting me, would render it impossible for me to be offended at the expression of some difference of opinion, even if such difference should turn out to be grave and irremovable. I am very sensible that, besides general dulness and heaviness of style, there is too much obscurity, in my Essay on Algebra as the Science of Pure Time, and one thing I am, and was, prepared to admit, nay, if it had seemed needful, to contend for, that Algebra does not require, for its foundation as a Science, any knowledge or conception of the actual succession of events, or of the relation of cause and effect; continuous progression appeared, and still appears, to me sufficient; but this, I thought and think, is the essential element in the conception of what I call *pure time*. Whether I am right in using this last form of expression is in a great degree, nay, almost wholly, a metaphysical question, in deciding which for myself I confess that I have been much influenced by study of Kant's "Pure Reason," and let me own that I am not prepared to decide, with you, that it is possible for a human mind to "imagine a given length to be instantaneously generated, no one portion of it coming into the thoughts before or after another," in opposition to the teaching of Kant, which seems to me to be confirmed by my own consciousness, that "we can think to ourselves no line, without *drawing* it in thought" (wir können uns keine Linie denken, ohne sie in Gedanken zu ziehen). Kant adds, "nor even" (we cannot even form the thought of) "*time* itself, except by *drawing* a straight line, to serve as its external construction, giving, however, attention only to the *process* of that synthesis of the manifold whereby we successively determine the inner sense, and thereby attending only to the *successiveness* of this determination" (und selbst die Zeit nicht [denken können], ohne, in dem wir im Ziehen einer geraden Linie, die die äusserlich figurliche Vorstellung der Zeit seyn soll, bloss auf die Handlung der Synthesis des Mannigfaltigen, dadurch wir den inneren Sinn successiv bestimmen, und dadurch auf die Succession dieser Bestimmung in demselben, Acht haben). I cannot say whether this passage was in my recollection when I was drawing up my Paper on Algebra, but I remember that a similar train of thought prevented me from yielding to the suggestions of some friends, who were of opinion

that without much impairing the statement of my own view I should be likely to escape much opposition if I contented myself with speaking of continuous SUCCESSION, or progression, without introducing the jealousy-exciting name of Time. At all events, to show how naturally my view of algebra falls in with Kantian views of mind, though I am not aware that it presented itself to Kant himself, or to any of his commentators or disciples (with whose writings indeed I am but slightly acquainted), before the publication of my Essay, I may mention that in a work labelled *Kant's Metaphysic of Ethics, translated by J. W. Semple* (octavo, Edinburgh, 1836), the dependence of not only "Arithmetic" but "Algebra," "the Calculus," &c., on the intuition of time, is familiarly spoken of, though without any reference that I have observed to my remarks, published the year before.

'But you are not to consider me as sworn to adopt the words of Kant; and, so far as *authority* goes, I gladly own that I concede great weight to yours, on any question respecting the Metaphysics of Mathematics. I remember with great pleasure my introduction to you in London, and am glad that you too have me in remembrance; your works, through presentation or purchase, have for the most part reached, and interested me.

'And I am, my dear sir, very truly yours,

'WILLIAM R. HAMILTON.'

'P. S. *May* 14.—As to Triplets, I must acknowledge that though I fancied myself at one time to be in possession of something worth publishing about them, I never could resolve the problem which you have justly signalised as the most important in this branch of (future) Algebra; to *assign* two symbols Ω and ω, such that the one symbolical equation

$$a + b\Omega + c\omega = a_1 + b_1\Omega + c_1\omega$$

shall give the three equations

$$a = a_1, \quad b = b_1, \quad c = c_1.$$

But, if my view of algebra be just, it *must* be possible, *some* way or other, to introduce not only triplets but *polyplets*, so as in some sense to satisfy the symbolical equation

$$a = (a_1, a_2, \ldots\ldots a_n);$$

a being here one symbol, as indication of one (complex) thought; and $a_1, a_2, \ldots\ldots a_n$ denoting n real numbers, positive or negative, that is, in other words, n dates, in the chronological sense of the word, only excluding outward marks and measures, and the notion of cause and effect.'

From A. De Morgan *to* Sir W. R. Hamilton.

'*January* 3, 1842.

'I have undertaken for the Astronomical Society to look at Hansen and yourself, with a view to see whether his mode of using the *elements* of an orbit has been forestalled by you.

'The only Paper I know of in which you have treated the passage from the general equations to the mode of developing them is the "General Method in Mechanics," *Philosophical Transactions*, 1834. Is there anything else which I ought to look at of yours on the same subject? if so, will you oblige me with a reference to the *locus in quo*.

'But should it so happen that you have studied Hansen's various Papers, and can tell me anything yourself on the point without new trouble, I shall be very glad indeed to hear from yourself what you think of any connexion of methods which exists between the two—yourself and Hansen.

'I duly received your note; I have since sent an *à priori* definition of A^B to the *Cambridge Philosophical Society*, which renders the new algebra all explicable *à priori:* thus $\varepsilon^{\theta\sqrt{-1}}$ is made a creature of definition, not of subsequent interpretation. It is of course the *à priori* introduction of what answers to the logarithm of a number, which I call the logometer of a line given in magnitude and direction.

'*Def.* 1. On OX lay down Nap. log. of the length of A; on OY lay down the arc (rad. unity) of the angle AOX: the line having these projections is the *logometer** of A.

'*Def.* 2. By A^B is meant the line whose logometer is

$$B \times \text{logom. } A.$$

* Every line has of course an infinite number of logometers, according as we use θ, $\theta \pm 2\pi$, &c., for the angle.

[The reader can easily supply for himself the ordinary figure of rectangular coordinates here referred to. Compare *Transactions*, Camb. Phil. Soc., vol. VII., part iii., p. 292.]

'For example; one value of $\frac{\text{logom. } (-1)}{\sqrt{-1}}$ is π; as thus

-1, { log. of its length 0, its angle π;

$\therefore$ logom. (-1) is line π measured on OY:

Division by $\sqrt{-1}$ is turning a line backwards through a right angle;

$\therefore$ $\frac{\text{logom. } (-1)}{\sqrt{-1}} = \pi$ measured on OX, whose symbol is simply π.

'There is nothing now which ever gives me any thought or care in algebra except divergent series, which I cannot follow the French in rejecting. But I feel confident that in time the full import of these things will appear.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 28, 1842.

'I was on the very point of leaving home for a short time when your note reached me, nor, though I have returned, can I yet say more in reply than that as you do not mention my *Second* essay in the *Philosophical Transactions*, it is barely possible you may not have seen it. Hoping to write soon again.'

From A. De Morgan *to* Sir W. R. Hamilton.

'*January* 31, 1842.

'Many thanks for your note. Since writing I have looked at your first [Essay on Dynamics]: the second I remember, now you mention it.

'We have awarded the medal to Hansen, and it will be given on Friday week. His line and yours are different; he has a curious way of making the time t into something else, and making this serve for variation of elements. Of course it only sets one thing right—longitude for instance; and he then corrects his new t to put the radius vector right. Airy calls it original, and there is no doubt he is a sagacious and painstaking man.

'Now if anything should strike you as to your *rapport* with him, it will be very good for our President's Address on Friday week; into which it can be incorporated. . . But you see that I need not ask you to trouble yourself as I have done, if your paths had been nearer together, to avoid mistakes.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *February* 8, 1842.

'Though hard at work just now upon another subject, I write a line to mention that my recollection of the nature of Hansen's researches entirely agrees with yours; and that though I regret not to have been able to give to them much attention, yet I know enough of them to feel sure that they well deserve a medal.

'My own investigations in Dynamics lay in quite a different direction; they conducted me to a system of rigorous and general expressions for the integrals of the differential equations of motion of any system of material points, attracting or repelling each other according to any function of the distance.

'I understand that Jacobi considered my results important, and believe that he has put them under a still more general form. Whether any allusion to them could be appropriately made on the occasion you refer to will be better judged of by others, who are on the spot, and know what is to be said about Hansen. Certainly there is no analogy of method, although the subjects are connected. My *Introductions*, in the *Philosophical Transactions*, contain a summary of what I have done, or attempted, in the matter.

'I am now writing out for the press, with some additions and illustrations, a Paper on *Fluctuating Functions*, which I communicated to the Academy in the summer of 1840, and which I have some hope will interest you. At all events I shall request your acceptance of a copy, when it is printed. It contains a new proof of Fourier's theorem, with generalised forms of that and other expressions for arbitrary functions, or parts of them.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*October* 11, 1844.

'I hope this will find you better in health than Graves represented you when I saw him last. The Cambridge Philosophical Society asks authors for abstracts now; accordingly I send you an abstract of a Paper which I have just sent down to Cambridge. You will see that you are concerned in the concoction, and that though you will not triplicise, yet "numero deus *impare* gaudet" may find followers.

'The first-mentioned system is rigorous common algebra for every plane passing through the axis of x. When two lines are not in one plane with that axis, still $AB = BA$; but when *three* lines are in different places, then $A\,(BC)$ is not $(AB)\,C$.

'Trusting to hear better accounts of your health, I remain, &c.

'If ever you looked, as perhaps you did for a while, to resolve

$$(a^2 + b^2 + c^2)\ (a'^2 + b'^2 + c'^2) = A^2 + B^2 + C^2,$$

you were trying the following :—

'"To find three points on a sphere each of which is opposite to *both* of the other two."'

'*Abstract of a* MEMOIR ON TRIPLE ALGEBRA.

'The extensions which have successively been made in algebraical interpretation have been consequences of efforts to interpret symbols which *presented themselves* as necessary parts of the algebraical language which is suggested by arithmetic. The now well-known significatien of $a + b\sqrt{-1}$ did not yield any new imaginary or unexplained quantities: and accordingly no effort (within the author's knowledge) was made to produce an algebra which should require three dimensions of space for its interpretation, until Sir William Rowan Hamilton wrote a Paper (the first part of which was published in the *Philosophical Magazine* before the present one was begun) on a system of "quaternions." This system, as the name imports, involves four distinct species of units, one of which may by analogy be called *real*, the others being *imaginaries*, as distinct from one another as the imaginary of ordinary algebra is from the real. These imaginaries are not deductions, but inventions: *their laws of action on each other are assigned.* This idea Mr. De Morgan desires to acknowledge as entirely borrowed from Sir William Hamilton.

Sir W. Hamilton has rejected the idea of producing a *triple* algebra, apparently on account of the impossibility of forming one in which such a symbol as $a\xi + b\eta + c\zeta$ represents a line of the length $\sqrt{(a^2 + b^2 + c^2)}$. Mr. De Morgan does not admit the necessity of having a symmetrical function of a, b, c; and, throwing away this stipulation, points out a variety of triple systems, partially or wholly interpreted.

'Sir W. Hamilton's quaternion algebra is not entirely the

same in its symbolical rules as the ordinary algebra; differing in that the equation $AB = BA$ is discarded, and $AB = -BA$ supplies its place.

'Those of Mr. De Morgan's systems which are imperfect all give $AB = BA$, but none of them (the imperfect ones) give $A\,(BC) = (AB)\,C$, except in particular cases.

'Mr. De Morgan gives systems of triple algebra which he distinguishes into quadratic, cubic, and biquadratic, according as the invented imaginary units represent square roots, cube roots, or fourth roots, of the negative real unit. It would not be easy in an abstract to give any account of them: but among them are found—

'1. An imperfect quadratic system strongly resembling the common double algebra, and which would, but for its imperfect character, be at once recognised as the proper and natural extension of the interpretation of imaginary quantities to three dimensions of space: the ultimate symbol for a line is

$$l\,(\cos\theta + \sin\theta\,\sqrt{_\omega - 1}).$$

'2. An imperfect quadratic system, very like the former one, except in having a peculiar inversion in the operation of multiplication, and a somewhat remarkable mode of representing what would by analogy be called arithmetical multipliers.

'3. A perfect quadratic system, the interpretation of which has considerable resemblance to that of the first-mentioned system, and is completely attainable, though not of great interest.

'4. Three perfect cubic systems, each irreconcilable with the others, though closely connected with them. Each system presents a triple trigonometry, the cosine and *two sines* of which are each a function of two angles; but these can be easily expressed as functions of common circular and hyperbolic sines and cosines.

'The interpretations of these systems are very imperfect, and appear to present great difficulty; but their symbolical character is unimpeachable.

'5. A perfect biquadratic system, which is of a redundant character; that is, its fundamental form represents a line drawn in space from a given origin, with a symbol to spare, which may represent the time of drawing it, its density, its tendency to a given position, &c., at pleasure.

'Many interpretations are attainable, but Mr. De Morgan does not pretend to say he knows the one which ought to be adopted. It is singular that every attempt to reduce this algebra, by assigning a condition among the subsidiary symbols of its fundamental form, leads to an imperfect algebra. The system first mentioned in this abstract is one such result, and fails in its rules of multiplication, as before mentioned. Another is obtained, which is perfect as to its rules of multiplication, but fails in rules of addition.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, Trinity College, Dublin,
'*December* 9, 1844.

'I must have appeared discourteous in not sooner acknowledging your very kind letter, written to me about two months ago; yet trust that you have not been displeased with the terms in which I noticed it in the supplementary number, for this month, of the *Philosophical Magazine*. Immediately after your letter arrived came an old friend [John T. Graves] to visit me; and if you have ever been afflicted with the disease of procrastination, you must know that a slight cause, preventing immediate action, may be sufficient to produce a long delay.

'I say nothing of slight attacks of ill-health, respecting which you are good enough to inquire.

'You must not say that I *refuse* to *triplicise*: I had made a great number of attempts in that way, of some of which I have *quite lately*, and *subsequently* to communications from you and other friends, begun to think that they might have been worth pursuing. But it is *too late* for *me* to *claim* any merit in the matter, or even to *fancy* myself entitled to any; I was prepossessed with an objection which is gradually melting away, and shut my eyes against some things which now seem obvious.

'Yet I cannot altogether regret this result, even as respects myself, if I do not err in my estimate of the prospects which may belong to my own quaternion-theory, when it shall come to be taken up by abler hands than mine. It will surprise me, I confess, if either your theory, or any other person's, of *pure triplets* shall be found to surpass that which I have been led to perceive, as *included* in my theory of quaternions, on all, or most of, the three following points:—

'1st. *Algebraical simplicity;* . . . analogy to ordinary algebra, as to the rules of addition and multiplication (the commutative property excepted);

'2nd. *Geometrical simplicity;* . . . ease of construction; the rule of the diagonal; and, above all, *symmetricity of space*, no one direction being eminent;

'3rd. *Determinateness of division;* . . . a quotient being never indeterminate or impossible unless the constituents of the divisor all vanish.

'Of all these assumed requisites, or things aimed at by me (and I admit that I aimed at others), what *now* appears to me most my own is the SYMMETRICALNESS OF SPACE in my system. If *you* have succeeded in representing this with pure triplets, *eris mihi magnus Apollo.* My *real* is the representative of a sort of *fourth* dimension, inclined equally to all lines in space.

'P. S.—May I mention as an example of the *working* of my system, what I mentioned last month to the Royal Irish Academy, namely, a solution of the (certainly easy) problem respecting the resultant of any number of forces, applied at a common point on the last side of a closed polygon?

$$v = v_1 + v_2 + v_3 + \ldots + v_n\,; \quad \therefore \quad v^2 = v_1^2 + v_2^2 + v_3^2 + \ldots + v_n^2$$
$$+ (v_1 v_2 + v_2 v_1) + (v_1 v_3 + v_3 v_1) + (v_2 v_3 + v_3 v_2) + \&c.\,;$$

but $v_1^2 =$ *minus* the square of the length of v_1; and $v_1 v_2 + v_2 v_1 =$ minus the double of the product of the lengths of v_1 and $v_2 \times$ the cosine of their inclination; $\therefore$ &c.

'I also gave a theorem respecting the composition of finite rotations, deduced with great ease from my general method. An *impossible line*, as a tangent to a sphere from an internal point, is often indicated, in my theory, by having a *positive square.*

'2nd P. S.—*To-day*, in looking over my old notes respecting triplets, I observed a recent one, dated the 25th of last September, written while I was thinking of going or writing to York, and intended as an argument *against triplets;* namely, that if we assumed i and j to be two such square roots of negative unity as to have their product = positive unity, but to be unconnected with each other by any linear relation, we should have

$$x'' + iy'' + jz'' = (x + iy + jz)\,(x' + iy' + jz'),$$

where

$$x'' = xx' - (y-z)(y'-z'),\ y'' = xy' + yx',\ z'' = xz' + zx';$$

my objection to which system, at the time, was this, that it gave $(i+j)(x'+iy'+jz') = (i+j)x'$, and therefore caused the quotients $(i+j)^{-1}$, $(i+j)^{-1}i$, $(i+j)^{-1}j$, to be all absurd, or impossible. (Of course I knew that in the ordinary theory of imaginaries the assumed equations relative to i and j gave $i+j=0$; but the spirit of the attempt in question led me to *exclude* that relation as being *linear*). But looking at the subject again to-day, in the light of a very interesting communication, received about a fortnight ago, from my friend John Graves (whom I authorised to give you a sketch of my views about quaternions, when writing first to him on that subject, in the postscript of my lately printed letter), I seem to see that it might not be wholly labour lost to develope the consequences of these equations of multiplication; and certainly not difficult to construct them.

'Through each factor-point, xyz, and $x'y'z'$, and through the product-point $x''y''z''$, conceive an elliptic cylinder to pass, of the form $x^2 + (y-z)^2 = \mu^2$ = square of what may be called the *modulus*; then $\mu'' = \mu\mu'$; the projections on either of the circular sections or on either of the planes of xy and xz (made by lines parallel to the indefinite axis of the cylinder) obey Mr. Warren's rule; and if we make $x = r\cos\phi$, $y = r\sin\phi\cos\psi$, $z = r\sin\phi\sin\psi$, we shall have the equation $\tan\phi\sin(\psi''-\psi) = \tan\phi'\sin(\psi'-\psi'')$.

'The modulus is the semi-axis major of the ellipse perpendicular to the indefinite axis of the cylinders.

'3rd P. S.—I would not, if I could, prevent the study of pure triplets; I feel for them something of the affection of a first love, for I was rather intimate with them once, though I have since been drawn away by what I thought superior attractions.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 16, 1844.

'I am much obliged by your note. I have carefully abstained from your quaternions till now, and shall abstain till I have corrected my proof, which is now before me.

'We are clearly on different tracks, and both necessary ones.

You are all for interpretation, and prepared to take new symbolic rules to get it; my object is strictly to keep the symbolic rules of common algebra, and to let meaning come if it will.

'I strongly suspect that you have the right sow by the ear, and that *easy* interpretation requires that the run of xy yz zx should be different from that of yx xz zy.

'My systems are not *well* interpreted, *except* by dropping (not changing) a symbolical rule. What may come of it I don't know; I had but a fortnight at it before a slight attack of illness came on, and when I was able to work again, my lectures claimed me, and I have not seen the subject again till this proof arrived.

'But I now see that your system is *triple*. You may say with Lord Byron

> "That you devoutly wished the three were four
> On purpose to believe so much the more."

I have a triple system just like it, with an additional undetermined agent. Graves says your real quantity is only a kind of agent [or sub-agent?*] upon the multiplications, &c.: I suspect my *biquadratic triple* system has some very strong affinities with your quaternions, though the positive interpretation will be very different.

'However this may be, I am convinced that any system which does business with rotations cannot have $xy = yx$.

'Graves gave me some extracts from your letter now published. His head ran on the transformations of sums of squares into other forms. He never dropped a hint about *imagining* imaginaries. On such little things do our thoughts depend. I do believe that had he said no more than "Hamilton *makes* his imaginary quantities," I should have got what I wanted.

'The system described by you with modulus

$$a^2 + (b - c)^2$$

is one of mine which I do not print, preferring one with a trifling variation which gives $a^2 + (b + c)^2$ $ij = -1$.

'I had described this system to Graves, as a person describes in conversation.'

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* Writing nearly effaced.

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*December* 30, 1844.

'I send you the proof of my Paper (done with; you need not return it), which will perhaps give you my view of the matter two or three weeks earlier than I could otherwise communicate it. When clean and presentable copies arrive I hope to supersede this ragged affair. I have no hope of being able to think much about the matter before July. I hope I shall then be able to satisfy myself about the proper interpretations.

'I look to what I have called the redundant biquadratic system* as giving the best chance.

'Have you ever considered the *vexata quæstio* of fractional differential co-efficients? We shall never arrive at the full comprehension of integration until we know what

$$\left(\frac{d}{dx}\right)^n . \phi x$$

means for all values of n. When divergent series are understood, and

$$\left(\frac{d}{dx}\right)^{\alpha+\beta\sqrt{-1}} \phi x$$

is as well known in all cases as when $\alpha = 1, \beta = 0$, we shall be in a very good state.

'Here is a quirk of analysis which was given in a Cambridge examination paper—

$$\frac{x}{y} = 2\left\{\frac{\sin x}{\sin y} - \frac{\sin 2x}{\sin 2y} + \frac{\sin 3x}{\sin 3y} - \ldots\ldots\right\}.$$

I should doubt its being true. It is a particular case of the theorem

$$\sum_{\text{from } n=-\infty}^{\text{to } n=+\infty} \phi\,(x+n).\,a^n = 0;$$

but this is only true when there is no discontinuity in the series

$$\phi\,x + \phi\,(x+1)\,.\,a + \phi\,(x+2)\,.\,a^2 + \ldots\ldots,$$

* *Supra*, p. 252.

which, I should think, would not turn out to be the case in the above.

'I hope you are well, and taking care of yourself. Nobody gives you a good character in the second particular.

'The Astronomer Royal in *this* country always lays down his work the moment he feels wrong, and plays till he feels right again. You have too much of our stock of science invested in your head to be allowed to commit waste. You are only tenant for life, and posterity has the reversion; and I don't see why you should not be compelled to keep yourself in repair.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 5, 1845.

'Many thanks for your *revise;* the final copy will also be welcome.

'Have you one to spare of your first Paper on the Foundation of Algebra? I know you sent me one, for (too rare event with me) I thanked you for it, and remember that I quoted Kant; but it is buried, I am sorry to say, among piles of pamphlets and papers, and is, for the present, lost.

'Is it too late for you to try whether you may like the following geometrical interpretation of your cubic system? In page 8, equations near the foot, θ is the dihedral angle between two planes, one containing the primary unit line (1, 0, 0), and the other the line (a, b, c), $\sqrt{\frac{2}{3}}\, l \cos\theta$, $\sqrt{\frac{2}{3}}\, l \sin\theta$, and $\sqrt{\frac{1}{3}}\, le^{\phi}$, are three rectangular co-ordinates. Let $a + b = 0$, $a + c = 0$, be called the equations of the *axis* of your system, and $a = b + c$ the equation of your *equatorial plane.* Then the following theorems are true as interpretations of your rules of multiplication:—

'1st. The projections of the unit-line, the two factor-lines, and the product-line on the *axis* are proportionals, in the ordinary sense of real or single algebra.

'2nd. The projections of the same four lines on the *equatorial plane* are also proportionals, in the sense of Mr. Warren's double algebra.

'At least I know that these theorems are true when we take as equations of multiplication those other forms which you mention:

$$A = bc' + cb' + aa'$$
$$B = ab' + ba' + cc'$$
$$C = ac' + ca' + bb'.$$

What put them into my head I may tell you another time, as I hope to write soon again. In great haste,' &c.

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 5, 1845.

'A medical attendant having called to look at my ankle, which is slowly recovering from a sprain, I write a line while he is wrapping something about it (holding the paper in my hands), to make sure of conveying rightly what I sent a while ago to the post in Dublin by another messenger, about the meaning of your θ, as I wrote then also in great haste.

'It is the dihedral angle between the planes passing through what I call the axis of your system,

$$x + y = 0,\ x + z = 0,$$

and containing respectively the lines (1, 0, 0) and (a, b, c). I have no copy of what I wrote just now, but hope that this was the meaning.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, *January* 12, 1845.

'I am much obliged to you for the interpretation in two notes. I have been so much occupied with the recommencement of lectures that I have not yet had time to consider it properly. I write this merely to say that I send the Paper on triple algebra by this post, since the receipt of the same without a line might make you think your notes had miscarried.

'I hope your ankle is better.'

From the Same *to the* Same.

'*December* 23, 1845.

[After giving a list of Hamilton's Memoirs, in quarto, in his possession, Professor De Morgan continues]:—

'. . . I shall be obliged very much if you will tell me at your leisure (for the thing does not press at all) whether this is all you

have published *in quarto*. And also whether you have ever appeared in octavo elsewhere than in the Notices of the R. I. A. and the Reports of the British Association.

'Should you have a spare copy of anything not above named, of course I should be very much obliged to you for it; but supposing your stock exhausted, the name and date will be of use—I dare say I shall pick up anything, if I know what to look for.

'If I were you (this by way of side appeal to your own authorial feeling) I should always take care to put anybody who is trying to be a bibliographe into possession of all correct information; for on the "harmless drudges" (as Johnson called lexicographers) must depend whether you go down in full or mangled.

'I have had no time for algebra this year—not even to read Mr. Graves's* Paper thoroughly—capital as it is. The "Symbolic Geometry" I have only seen, not read yet; but I am glad to see it.

'I hope you are in health and with good will to your work. You must have a good many shots in the locker.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, Trinity College, Dublin,
'*February* 2, 1846.

'More than a year ago I made a communication to the Royal Irish Academy, in which your Triplets figured. The notice was in type in June last; for I received and corrected "proofs" in that month; but separate printed copies have never since been received by me. The hope of receiving them has been an excuse to my conscience for not writing sooner to you; allow me now to send you the manuscript notice, such as the printers returned it to me last summer, with all its faults of hasty writing and semi-erasures on its head. I shall be much gratified if you accept it, in that state, as some small payment in kind for the proof-sheets of your important Paper on Triplets, which you sent me in 1844. You have probably seen, and I presume have received from the author, the notices of the communications made to our Academy here by the Rev. Charles Graves on the same general subject last year: they pleased me much, and you will find that your priority is acknowledged, on several important heads.

* Professor Charles Graves: see *infra*.

'For my own part, I have not been exclusively occupied by my Quaternions, but confess that they have been growing in interest upon me, and that I more and more believe they will one day justify a hope which I ventured to express in an Address to the R. I. A., on the first night of its session of 1844–5, namely, that they will constitute nothing less than "a new algebraical geometry." Of that Address, or oral communication, which was somewhat extemporaneously made, other authors choosing to yield me precedence at the time, I am fortunate enough to possess the power of giving some printed notices to scientific friends, having ordered the printers to strike off some at my own expense before I went to Cambridge last June; at which University I gave several copies away, but have several still remaining, of which I shall be happy, if you will allow me, to present you with one.

'The *words* above-mentioned do not, I believe, occur in the printed notice, but it is a very fair statement (drawn up indeed by myself) of the substance of the communication which I made in November, 1844. Some later communications have been made by me since to the Academy, on the applications of Quaternions to Problems of Dynamics and to Surfaces of the Second Degree.

'If I have alluded to delays in the publication of the *Proceedings* of the Academy, it is only fair to allow that, besides my own personal dilatoriness, I may, though not the editor, be in some degree to blame for not more strongly urging punctuality as President. But I hope that some allowance may be made for my unwillingness to have the air of coercing persons even more dilatory, and at least as scientific, as myself.

'I did indeed procure the passing, about a year ago, of a self-denying ordinance, by which any author who was not ready with his abstract in its turn was to submit to be relegated to an Appendix, and have acted or suffered thereupon, with reference to some of my own communications of last session. The *Proceedings* of that session have been all (with the exception of the said Appendix) for some time in print, and I heartily wish that they were published; but believe that the editor desires to suppress a certain controversial correspondence, which can scarcely interest any beyond the circle of our own members, though it could do us no discredit. (It is one which in no shape concerns me personally.)

As to myself, you may perhaps be aware through some extracts

from the Dublin papers, or if not, let me be now allowed to inform you, that I gave notice at the beginning of the present session of an intention (which had been formed after a very careful deliberation on my part) to retire from the Presidentship at the annual election next month (on the 16th of March). I hope that my tenure of the chair has not been wholly useless to the body: certainly the Academy is now, in numbers and energy, as well as in collections, more rich and flourishing than it was when I was elected in 1837, however little my exertions may have contributed to such a result. But I feel it to be quite necessary that I should have more leisure for scientific study than the labour, and still more the cares of the office, have for several years allowed me; and it is very pleasant to me to think that if the Academy shall have any difficulty respecting my successor, it will only be to make a choice among several persons eminently worthy.

'I have not your last note beside me at this moment, but will not let that circumstance serve as an excuse to me for any further delay. The accompanying Paper, on the earliest printed Almanacs, was very welcome, especially as I had been much interested a year ago in the question about the time of Easter, and much amused by some of the newspaper correspondence on the subject. Indeed I was applied to by an Irish clergyman of some general talent, but who had no acquaintance with the subject, and with whom I was not myself personally acquainted, to give a decision *ex cathedrâ*, as Royal Astronomer of Ireland. I took some pains with my reply, chiefly that I might convey without discourtesy my view that he had no right to consult an astronomer on the subject at all, but was "concluded" by the tables in the Prayer Book (published, I believe, as they now stand, in the Act for change of style?) as the authoritative voice of the Church and State in the matter. And I remember that among my illustrations of the inconveniences which would have resulted from the absence of such authoritative and technical directions, I took some, with acknowledgment, from a letter of yours to the *Athenæum*. At the same time I pointed attention to a rubric, connected with one of the Tables (the one for the 20th century), which had not, and has not, been noticed, so far as I know, in the controversy, to show that a difference between the "ecclesiastical" and the "real" full moons was perfectly well known to exist. My worthy

correspondent has never favoured (or troubled) me with an answer.*

'I feel some curiosity to know how the Moravians settled the point, and shall perhaps consult an excellent old uncle of mine, who is a retired Moravian minister, and with whom I correspond occasionally on things connected with astronomy (the Rev. John Willey of Gracehill). He has a passion for numerical calculation, and is just now in raptures with a work of Burckhardt's, of which I was lucky enough to ascertain the title for him.

'Are you aware that the Equinox in 325, A.D., fell on the 20th, not on the 21st of March? Some very simple mental calculations led me a few years ago to doubt the latter date, though asserted in, I cannot say how many, books on astronomy; and I then settled the point at least to my own satisfaction at the time by a careful computation with Vince's and other Solar Tables. I wish you would enlighten me as to the history of this widely-spread mistake.

'P. S.—I am happy to say that I have at least one Paper of some elaborateness, which, according to my recollection of your last note, you do not seem to have ever received, but of which I could put even two or three copies at your disposal. It is the "Second Part" of my *Essay on a General Method in Dynamics.* I find that it is quite possible, and not difficult, to connect the general formula of that Essay with Quaternions.

'2nd P. S.—I have been dreadfully remiss in writing not only to you, but to my old friend John Graves: you may, it is not unlikely, meet him before I write; in that case give my very kind remembrance, and tell him, if you please, that I have reserved for him a copy of the Abstract which I brought with me to Cambridge.'

From A. De Morgan *to* Sir William Hamilton.

'7, Camden-street, Camden Town,
'*February* 15, 1846.

'Your note was not to be answered off-hand, and when I received it I was as busy as a person need be in concocting an Annual Report for the Astronomical Society.

'But now it's read, and that's all over, and I shall neither lay

* See vol. ii., pp. 482-4.

down and cry nor die in consequence. In fact I made a memorandum to survive to answer yours.

'First, I thank you for the notice both as to matter and manner. The former I shall con in detail the first time I can get again on triplets; and the MSS. will bind up with the other things very nicely, and increase the value of the volume.

'I was very much gratified with C. Graves's Paper, but, as before, have never found the opportunity to try it as I could wish. But it is a finished thing. I rather suspect there is another; but that one is that one, and must continue to be so.

'The Quaternions will, I have no doubt, make a system, in which *rotations* round the three axes play the part of co-ordinates, complete in itself. I shall be very much obliged to you for the notice you mention—that which you distributed at Cambridge. I heard speak of it, but did not see it.

'I was glad to hear that you are going to resign the Presidentship. You have no business there at all—there are plenty of people who can do all that a President, as such, has to do; and I maintain that any man who is fit for original research has no business to be a president or secretary or treasurer, *at the expense* of his researches.

'Now for Easter. I am not sure, by your note, that you saw my first communication to the *Companion to the Almanack* on this subject, containing the development of what you mention in the *Athenæum*. I therefore send one. Should I have sent it before, you can find some one to give it to—your uncle, for instance, when you ask him for the Moravian determination.

'You got him Burckhardt; no doubt B.'s table of prime numbers up to 3 millions odd. But B. only gives the fact of primeness, or, when not prime, the lowest divisor next above 1. But Chernac *Cribrum Arithmeticum*, Daventriæ (Deventer), 1811, 4to, gives, up to 1,020,000, every prime factor, which is more convenient.

'As to the equinox of 325, it is a not very well known fact that using the Alphonsine Tables to recover Easter of the Nicene Council, the Gregorian Committee made a mistake of a day, and concluded that the equinox ought to vibrate over the 21st and 22nd, when in truth it did vibrate from the 20th to the 21st. Their equinox of 325 was a theoretical one formed from their own Tables. The mistake hangs on to what I have been pointing out

in what I send and sent, namely, that a specific astronomical code has been forged for the Nicene Council, though not one word was said by them except "Easterns, keep Easter as the Westerns do." But what the Westerns did—whether it was moon, sun, or neither, which regulated; whether even the Council knew the difference between the Sun and Moon—cannot be gathered from what they said of Easter, or what any contemporary historian has said. No wonder that the equinoctial 21st should have been fastened on them, as well as the cycle of Dionysius, or anything else which was made after them.

'I have not seen Graves lately, but hope to do it soon. I shall be much obliged to you for your second part of Dynamics. I now remember to have seen it; but when I wrote to you I mentioned nothing but what was before me. But none of your "some elaborateness." I want a complete list; and when my note turns up again pray answer me categorically, as they say; but there is no hurry. In the meanwhile, if you will just send the second part of Dynamics and the Cambridge Abstract, and Graves's copy of it, which you mention, in a railroad parcel, addressed to me at "University College, London," I shall be very much obliged.

'I have been trying lately at Arbogast's method of derivations, and find that there is much to learn out of him.

'The hindrance in the way of development (I mean of organized rules for it) appears to have lain in the imperfect view taken of Taylor's Theorem.

'If we write

$$\phi\,(a+b) = \phi a + \phi' a \,.\, b + \phi'' a \,\frac{b^2}{2} + \ldots,$$

the 2, 2·3, &c., stick in the way of describing these terms as formed upon a repetition of one process. But if we write it as

$$\phi a + \frac{d}{da}\int_0^b db\ \ \phi a + \frac{d^2}{da^2}\int_0^b db\ \Big\}^2\ \phi a + \ldots,$$

we see that there is one operation which, being successively performed on ϕa, will produce the successive terms.

'Derivation, the real tool of development, is not a mere differentiation, as Arbogast would at first have made it, but a differentiation accompanied by a subsequent integration; and in the series

$$a + bx + cx^2 + ex^3 + \ldots.$$

the tools to be used in developing functions of it are

$$\frac{d}{da}\int_0^b db, \quad \frac{d}{db}\int_0^c dc, \quad \frac{d}{dc}\int_0^e de, \text{ \&c.},$$

the successive uses of these giving what Arbogast calls his *dérivées divisées*—ought to have been his *dérivées*.

'I have sent this view of the matter, with a new mode of demonstration, to the *Cambridge Mathematical Journal*, where, by the way, I saw your Algebraical Geometry. A little more attention to this subject would perhaps extend the calculus of operations. Arbogast's divided derivation (call it D) gives

$$\phi\,(a + Da \,.\, x + D^2a \,.\, x^2 + \ldots) = \phi a + D\phi a \,.\, x + D^2\phi a \,.\, x^2 + \ldots,$$

in which a, Da, D^2a, are independent. This is equivalent to saying that if ∇ be the operation

$$1 + xD + x^2D^2 + \ldots$$

and ϕ any functional symbol; then

$$\phi\,(\nabla a) = \nabla\,(\phi a).$$

But it is not true that

$$\phi\,(\nabla^2 a) = \nabla^2\,(\phi a),$$

for reasons connected with the effect of ∇ in introducing new and independent subjects for the second ∇ to deal with.

'But it grows late, and I must therefore conclude.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 12, 1847.

'I send you a Paper on Logic, out of which will arise a question of literary piracy. I wish you would give me your opinion on this point (I, you understand, am the asserted pirate).

'Looking at § 3, *on the quantity of propositions* (admitted to be mine), what hint did I need to write thereupon the first two pages of the addition at the end?

'The party making the charge is your namesake, Sir William Hamilton of Edinburgh. If I cannot drive him to press in a week

or two (which I am trying at, but he does not answer the spur as well as a man ought to do who has made such a charge—but then his health is not good), I must publish myself; so that all will soon be out.

'In the meanwhile I send you this information that you may not stare if anybody tells you that *you* are charging me with stealing logic from you. I shall take every possible care to identify my man and distinguish him from you; but I know it will not entirely succeed, for you are constantly confounded with the Edinburgh Sir W. H.

'I was talking to a friend on this matter the other day, and I said to him, "You know Sir W. H. is no mathematician—in fact he is an opponent of mathematics." I saw my friend's eyes open very wide, and he looked to see if I were gone mad—I had forgotten to say "of Edinburgh."'

From Sir W. R. Hamilton *to* A. De Morgan.

[from a copy corrected by sir w. r. h.]

'Observatory, Dublin, *May* 7, 1847.

'I have received your two recent communications, and think you have taken all reasonable precautions against my being confounded on the present occasion with my celebrated namesake of Edinburgh. I daresay it would be *nuts* to you to have two Sir William Hamiltons on your hands as controversialists at one time; but this note is to warn you that I don't think I shall indulge you on that point. Perhaps you may ask, What provocation have you given to such a simultaneous controversy? A very gentle one, certainly, and not very recent, but one which I *might* have a good opportunity of now accepting, if I were disposed, which I am not. My manuscript researches respecting Quaternions, and their applications to Geometry and Physics, having attained a considerable extent, and a number of scattered notices, themselves by this time not very small in bulk, having been printed, I am urged by my friends in Dublin, and am myself now desirous, to make at least a *beginning* of that more full and formal publication which I have all along intended. After many hesitations as to whether I should not at once proceed to the parts which are more likely to interest and not to shock mathematical readers in general, I have decided on

following a more historical order; and have handed in to the Committee of Publication of the Royal Irish Academy, who have transmitted to their printers a Paper entitled *Researches respecting Quaternions, First Series;* in which Paper I have endeavoured to insert nothing with the principles of which I was not familiar at the time of making my first communication to the Academy on Quaternions, on the 13th of November, 1843. A good part of this Paper has been lying by me for some considerable time, and on the whole it is, I think, very nearly what I would have drawn up in 1843, if our rules had been stringent enough to oblige me as an author then to hand in at the time a MS. prepared for being put into the printer's hands. The *word* "Triplet" does not occur once in this Paper, but the word "*Set*" presents itself very frequently; because I had in fact been familiar with the conception of sets, as including the conception of couples, for at least nine years previous to my perceiving my own *definite* system of Quaternions in October, 1843, and had announced an intention of publishing hereafter a theory of triplets and *sets* of moments, steps, and numbers, which should include the theory of couples, when I published (about August, 1835) in the xviith. vol. of the *Transactions* of the Royal Irish Academy, my Paper on *Algebraic Couples*, and on *Algebra as the Science of Pure Time.* See the concluding sentence of my Essay on that subject in that volume.

'Now for the controversy which I think is *not* to take place. It does *not* relate to the Triplets, on which my old unpublished and rejected researches cannot and ought not to interfere with your priority. But you may remember—or may not, for an author so original and fertile as yourself has room for forgetting many things of his own which other people find it worth while to remember—that in the first of your Cambridge Papers *On the Foundation of Algebra* you expressed, though very politely, a certain degree of dissent from my general philosophical (or if you choose *un*philosophical) view of the subject. I am conscious of having expressed that view, such as it was, obscurely at the time, nor have I much hope of being able to express it more clearly now, without taking more trouble and occupying more room than can perhaps be well spared from other things at present. But as I have not yet rejected it, and as the Quaternions did really arise in my own mind one day that, being then fresh from a reperusal of my old Essay, I

renewed my attempts to combine my general notion of Sets of Numbers, considered as suggested by Sets of Moments of Time, with geometrical considerations of points and lines in tridimensional space, it has appeared to me to be the most natural, clear, and honest course, to print, as the *First Series* of my *Researches on Quaternions*, an account of the manner in which the mathematical notion of *Time* leads (in my mind at least) to a *general* conception of Numerical Sets, which has by me, as yet, been only exemplified, in anything like a satisfactory and definite way, for the two cases of Couplets and Quaternions. Respecting the geometrical applications, the printers have in their hands for that First Series, *less* than I did actually communicate in November, 1843, by speech, and by large diagrams which were then exhibited to the Academy, because I am reserving most of the *geometry* for the *Second Series*, communicated in November, 1844; to be followed by a *Third Series*, of a more *dynamical* character, of which sketches were given to the Academy in 1845; and these, too, probably by others with the list of which I forbear from now alarming your patience. And although it is likely that I shall append to the *First Series* some general remarks, as yet unwritten, and to be dated, as an appendix, according to the actual time of writing them, which will probably not be until the printers are actually ready; yet I do not think it likely at present that I shall write anything of a controversial character in such appended and general remarks, which abstinence from controversy, if it be realised, will not I hope be accounted disrespectful by you. Metaphysical and logical speculations have a great charm for me, but in a certain sense and degree my mind is (I think) less analytical than synthetical; that is to say, as bearing on the present subject; whenever I catch, or fancy that I catch, a glimpse of a principle, I am impatient to *apply* it—not exactly towards the making of a railroad, but still to apply it in some way of my own. Thus, I like better to work out my notion of Time into its mathematical consequences, than to enter into any *d priori* discussion whether it be metaphysically correct, though I have speculated on that point too. And without pretending to settle by any clear *definition* beforehand *what* symbolical geometry should be, I have been gradually working into shape, by trial upon mathematical questions, my *idea* of symbolical geometry.

'Unlike as my little Papers on this latter subject in the *Cam-*

bridge and Dublin Mathematical Journal may appear to those other Papers which I have hitherto printed on Quaternions, yet, if you have dipped into both, you will not long fail to recognise, perhaps may have recognised already, that the "Geometrical Fraction" of the one set is just the "Quaternion" of the other in disguise.

'I go, you know, very seldom to London, but was there for a while last August, or at least at Clapham, where I was leaving my eldest boy at Mr. Pritchard's school. Your residence was rather far, but I several times intended to call, though not sure of finding you at home; and the last thing I did before returning to Ireland was to make up a small packet of a few printed Papers, which I had designed to hand you, and which (I hope) reached you by post soon afterwards. I am far from being entitled to expect others to acknowledge the arrival of such things from me, since I seldom expressly and at the time acknowledge the many valuable Papers which I receive from other authors; but if you are writing to me on any other subject, I should be glad to know whether you received, about Christmas last, a larger octavo pamphlet on Quaternions, and a little Paper in January, *On the Law of the Circular Hodograph*—to which I have since been able to add several general theorems—the Quaternions which have long since become a calculus (= hobby ?) in my hands assisting me most materially in the investigations. My reason for asking is that I would try to forward *other copies*, if those former ones did not arrive. I remain, &c.

'P. S.—I make up a printed copy (first sheet of No. 50 of *Proceedings*) of some of Charles Graves's remarks on Triplets, and of my Interpretation, &c.

'Received a long and interesting letter from you about March or April, last year. Unlucky enough not to be able to find your Paper on Triplets—perhaps I may like to refer to it in my "Appendix" to the "First Series"—at all events am reading up in several directions, and should be glad even of the *loan* of the Paper. However, I am much interested at present in some applications of Quaternions to Physical Astronomy.'

From the SAME *to the* SAME.

'OBSERVATORY, *July* 10, 1847.

'On Thursday evening, July 1st, about 7 or 8 o'clock, at the Euston Hotel, London, near the railway station, before starting for Liverpool (or rather for Birkenhead), on my return homeward from the Oxford Meeting, I directed to your residence (7, Camden-street, Camden Town) a quarto copy of my *First Series* of *Researches respecting Quaternions*, and paid sixpence for its safe delivery; I therefore hope that it has reached you. If I had remained in London even one clear day, instead of remaining only an hour or two, I would have waited on you with the Paper.

'Am I wrong in thinking, or feeling, it to be somewhat remarkable that a false alarm respecting a relative of mine, arising from a mistake of names, should have hurried me home from the Oxford Meeting in 1832; and that a true account of the death of the same aged relative, whose funeral in Ireland I was in time to attend last Saturday, should have again caused me to hasten home from another Oxford Meeting in 1847; each time, it is true, just about the close of the Meeting?—the relative in question having been generally a remarkably healthy man, and no uneasiness felt about his health until extremely recently, except when a false report reached me at Oxford, at the time above alluded to, that he had been attacked by cholera fifteen years ago. It shows perhaps a predisposition to superstition that the coincidence should strike me as it does.

'I have the strongest hopes that the Quaternion Calculus will simplify physical astronomy, especially in all those departments in which spherical trigonometry is now employed. But as yet I don't pretend to have done more than to have proved that my Calculus does really take hold of the subject: that it is adequate to work out new and true expressions for the perturbations of the moon and planets which agree with known results, when translated into the known forms of language. Whether a sufficient compensation is afforded for the trouble of acquiring some new habits of calculation, and giving up some old ones, I am not an impartial, and therefore not a competent judge. But it is my business to multiply the materials for others to form their judgment upon; and this, if it be an arduous, is also a delightful task. The

freshness which old subjects receive, at least to me, from my new view of them, is a charm sufficient to repay me for any amount of labour.'

From A. De Morgan *to* Sir W. R. Hamilton.

'*July* 12, 1847.

'I have received the Paper, for which thanks. When I am a little out of my logical undertaking I hope to read it attentively.

'I hope you remember that you have to send me the *second part*, I think, of your dynamical Paper, which, apropos of the list of your works which I sent, you found I had not got.

'Your coincidence is a curious one—that your relative should never be either truly or falsely said to be dangerously ill, except to hurry you from the Meetings at Oxford alone. There can of course be no discoverable connexion between the two things. That there *is* no connexion is more than I know or you either.

'I am myself past thinking anything too extraordinary to be true. If we knew everything, should we or should we not find that all things are connected? that every action of every man that ever lived is connected with every action of every other man that ever lived?

'We know that every motion of every particle of matter has its effect upon the motion of every other particle.

'If the law of attraction be veritably and physically true, such must be the case. One man was so staggered by the idea that his snuff attracted the snuff in the Saturnian snuffboxes, that he wrote a book against gravitation.

'Now is there a *mental dynamics*? I can't tell. In these matters I can admit the possibility of anything, as long as a man says he can't prove it.

'There are two things which want a good deal of consideration

The action of moonlight,
The stories of ghosts.

I never reject the whole world's opinion entirely. In all nations it is a maxim that there are certain things not to be done in gardening when the moon is growing; and in all nations there are independent assertions of phenomena marking the deaths of friends to the absent. The two things stand alike; there is for

both independent and universal tradition, constantly affirmed to be reinforced by fresh observations; for both, vulgar exaggeration; for both, the almost universal rejection of philosophers; for both, continual exceptions to the rule of rejection on the part of educated men; for both, cases of isolated facts, in the evidence for which the flaws seem to be invented, not discovered. And so I leave them both.

'I have almost forgotten all about triple algebra. I have no doubt of the applicability of the quaternions. How can a complicated and self-consistent system fail to represent complicated things with *relative* ease?'

From the SAME *to the* SAME.

'*August* 17, 1848.

'I am glad to hear of your generally better health—report, backed by Graves, asserts it.

'Now to my subject. Long time ago I wrote you a list of your own works in my possession and asked if it was complete, to which you replied, "No, the *second part* of the Method of Dynamics is wanted, which I will send you." Now, if you made a Jesuitical reservation in your own mind and added to yourself—When the Union is repealed—all I can say is, that such reference *in Kalendas Hibernicas* leads to odious comparisons; and I think to myself, Why the other Sir William Hamilton would have used me better than that.

'Here have your tracts been lying in a heap, of which I am now sending twenty volumes to the binder, when they might have been vertically shelved in all the honour of leather and the glory of gilding.

'Pray think of my case, or rather of the impossibility of putting a case on the matter of this letter. And if you cannot find a copy, send me at least a reference to the volume of the *Philosophical Transactions*.

'For as to binding an imperfect set—a bibliographical conscience revolts at it, and an Irishman in these days ought to sympathize with revolt of any kind. Or if you are ever so strong a loyalist, you ought to allow your tracts to feel a sense of obliga-

tion as well as yourself, which they cannot do as long as they are unbound.

'I suppose this letter will find you on your return from Swansea, if you are there.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *August* 21, 1848.

'I am really flattered by your wishing to have that copy of my Second Essay on a General Method in Dynamics. I own I think it has some value, but one thing puts another out of my head. After some years, or possibly even sooner, if my health, which is quite strong at present, lasts, I may be induced to resume the subject, with such lights as later reflection may have given, but I feel that it will be a shame if I do not publish, after some time, a quaternionic treatise on mechanics; for *I* account quaternions an *instrument*, and not a toy.

'Wonderful to relate, however, the quaternions have been almost entirely out of my head for the last month or six weeks, but I am going to attack them again, and indeed Charles Graves has been pressing me to produce some *book*, large or small, on the subject, which he can desire mathematical students in Dublin to *buy*. The little interruption to my thoughts, which has arisen partly from some private causes (including the illness of a relation), and partly from some anxiety about the state of this country, may turn out to have been of service to me.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 28, 1849.

'Mr. Erck brought me your Papers, for which thanks. I have asked him to come to the Astronomical Society, where he will find an atmosphere full of notions which will do him good. He seems to have an idea that a man with a long head and a short equatorial can do nothing to the purpose: he must be taught better. We have men here, or have had, who would have been all the better of shortening their telescopes, if the piece cut off could have been added to their working hours. What do you say, as a metaphysician, to a piece of length cut off and added to time?

'Tell the boys that I am quite of opinion that *co- co-* is 1, or rather that a *co- co-* is $(co\text{-})^2$. But I should like them to explain this: How is it that *co- co*^{a} drinks as well at a round table as at a square one? And how is it that the cuckoo is not a square bird, but remarkably squat? Is it the little variation of spelling that does all this, or has it rather reference to change of meaning? This is a puzzle for them. I had no idea you had sons old enough to be trigonometrical.

'I have often used the phrase co-cosine in my lectures.

'I am printing nothing at this moment, save a solution of the old trial equation

$$x^3 - 2x = 5$$

done by a pupil of mine, *and verified by* another euqation, to 103 places. To wit,

$$x = 2{\cdot}094551481542326591482386540579302963857306105628239180304128529045312189983483667146267281777157757859 +.$$

Another tried 150 places, but broke down at the 76th, which was wrong.

'There is a great deal to be done in organization of processes—but not after midnight—so I subscribe myself, the right to date as below* having just accrued.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*August* 30, 1849.

'You are perhaps aware that, in the new arrangements for the Colleges, Young has lost his situation at Belfast; but you may not perhaps be aware that it has utterly ruined him, and that he and his family will probably be without any means of support.

'I hope you know and think enough of his long exertions to be willing to use what you have of influence—which ought to be something in such a matter—to induce Government to do something for him, cut out as he is by their means from his subsistence. I have written to Mr. Tennent, the Member for Belfast, whom I

* Professor De Morgan dates his letters at the end.

knew long ago, and should he feel able to make any application I hope you will feel able to back it. It seems to me a very cruel case, and one which might easily be avoided, for I should suppose there are many things within Government patronage for which Young is quite fit.

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *September* 24, 1849.

'I hope you received what was, almost literally, only a *line* from me, in answer to, or at least in acknowledgment of, your letter respecting Professor Young. Although, having never been consulted as to any of the details of the new Colleges, I have not made myself so far acquainted with them as, even yet, to understand *how* Professor Young has *lost* any situation by the new arrangements; yet I am too well informed, through Mr. Spiller and through you, of the *fact* of his being deeply injured, to have any doubt on the matter.

'At the very moment when your letter arrived I was deep in conversation with Mr. Hargreave on the subject; and had just been asking him to join me and others, in backing any memorial to Government which may be drawn up by any competent hand in favour of Professor Young; which he expressed himself as willing to do. I mean the mathematician, medallist, and now Commissioner Hargreave, who lately made me a visit. *His* notion was that the new College in Belfast could only injure Professor Young by tending to introduce a formidable rivalry; but there must, from what I hear, be something more injurious to him than this, though I cannot yet understand *what* it is. I shall be most willing to join you and others in backing his claims on the Government—with whom, however, I have no pretensions to exert *influence*, although I have always met with *courtesy* at their hands.

'I have to thank you for a former note respecting Mr. Erck, who was much obliged by your attention, and expressed himself to me as having not only enjoyed but profited by his visit to London.

'I remember that in a hasty line, forwarded by him, I said something to you about "co-," as a symbolic square root of unity. More lately, I was assisting my eldest boy, who is more than

fifteen now, to form the equation of a perpendicular by co-ordinates; and he saw clearly that if two perpendicular right lines through the origin have for equations

$$y = tx, \quad y = t'x,$$

and if we write $t' = ft$, we shall have also $t = ft'$, and $\therefore f^2t = t$, $f^2 = 1$. But here he was disposed to think that we must have $f = \pm 1$; whereupon I took the occasion to give him some notion of the Calculus of Functions, and mentioned your work thereon. Dr. Peacock remarked to me four years ago that Functions and Quaternions have several analogies . . . I have, you know, *infinitely many* solutions of the equation $\rho^2 = -1$; namely, those included in the form $\rho = ix + jy + kz$, where xyz need only satisfy the one condition—

$$x^2 + y^2 + z^2 = 1.'$$

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*September* 26, 1849.

'The manner in which poor Young is ruined is as follows:—The Belfast Institution, in which he was Professor, was supported partly by Government allowances, partly by pupils. On the foundation of the new Government College, the Government allowances are withdrawn, and the managers dissolve the Institution. So that his situation is in fact abolished—and directly by the act of Government.

'Any one would have supposed that a man of his name and worth would have been allowed to step from one place into the other. But poor Young is no partizan; and in the focus of religious dissension in which he lives such a person has no friends.

'Hargreave is my old pupil, and is in every point of view an acquisition to the acquaintance of anyone.

'What has not analogy with functional calculus? In fact algebra itself is a functional calculus. The *a*'s and *b*'s are, if you like, symbols of operation, not of quantity, the subject of operation being the concealed unit.

'I send you a few loose thoughts:—

'1. If $f(x, y, z)$ be homogeneous with respect to the three

letters, we know that

$$f(x + a,\ y + b,\ z + c) = 0$$

is a cone. But we know that if a, b, c be infinite, this cone is a cylinder, and has for equation

$$f(px + qy + rz,\ p'x + q'y + r'z) = 0.$$

How are we to show by pure algebra that the first form takes the second, when a, b, c are infinite, or any one of them?'

[The remaining "loose thoughts" are concerning—

2. The number of solutions of the indeterminate equation $ax + by = c$, and the limit for $c = ab - a - b$ beyond which the equation always admits of solution.

3. The meaning of the *total area* of a closed circuit, in the case of a curve which intersects itself any number of times.*

'I find the result which follows:—

'Take a point A (outside the whole circuit is most convenient, but the modification of the rule for an inside point is easy) outside the circuit. Take the element (infinitely small) of which you want to settle *how often* it goes positively into the area, and how often negatively, and draw a line through that element and the outside point.

'Name a positive and negative direction of revolution: go round the circuit, and every time you cross the *test-line* mark the direction in which you are revolving round A. Take the balance of + and − on *one side* of the element to be tested (either side will do). Suppose the balance is 3. Then the element, say dV either enters as $+ 3dV$ or $- 3dV$. Take every element, either choosing your balance always on the off-side from A, or always on the near side, and integration gives the area; + in one case, − in the other.

'The theorem is, that though your circuit is as long and as tortuous (you make parentheses, and so can I: I never began but one chancery suit, and that in the court of worst reputation of all, the Irish; and the moment the bill was filed, my party produced his accounts, and altered every unsatisfactory item on demand) as

* In the *Cambridge and Dublin Mathematical Journal*, 1850, Vol. v. pp. 139–142, there is a Paper by A. De Morgan on this subject, dated October 9, 1849, as is indicated in his letter, dated October 11, 1849.

a Chancery suit, the balance is always the same number for the same element.*

'4. This area question would require what may be called a swing-swang integration

$$\tfrac{1}{2}\int r^2 d\theta,$$

where $\int$ signifies integration forwards from α to β, back from β to γ, forwards from γ to δ, and so on.'

This applies to the question of the greatest area on a given chord bounded by a given length of arc, in the cases that the circular arc which is found is greater or not than a semicircle.]

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 3, 1849.

'If I lay a letter out of my hands for a few hours, without answering it, I am sure to find that it has been swept away and covered up, for the time, by the Charybdis of my other papers. No doubt, every such missing treasure may be expected, at some future time, to emerge to view; and may then be suddenly seized, by a bold and ready hand. Thus, from month to month, or at least from year to year, I find a note or two of yours eddying upward to the light; but, for the instant, your *last* long (and welcome) letter is invisible. However, I remember much of its contents, and shall send something now in answer to *one*, at least of its "loose thoughts."

'If we have a homogeneous equation

$$f(x, y, z) = 0,$$

we may put it under the form

$$f'(px + qy + rz,\ p'x + q'y + r'z,\ p''x + q''y + r''z) = 0,$$

where $pqrp'q'r'p''q''r''$ are arbitrary but constant co-efficients, and f' is still a homogeneous function of three variables; or we may write

$$f(x, y, z) = F\left(\frac{px + qy + rz}{p''x + q''y + r''z},\ \frac{p'x + q'y + r'z}{p''x + q''y + r''z}\right),$$

where the function F is *not* obliged to be homogeneous. And

* Note that in a real Chancery suit the balance generally varies with the point of view from which the case is looked at.

nothing hinders us to assume

$$F(\zeta, \eta) = \phi(t\zeta, t\eta),$$

when t is a new arbitrary multiplier, and ϕ is a new arbitrary function of *two* independent variables. Thus we shall have the transformation,

$$f(x, y, z) = \phi\left(t \cdot \frac{px + qy + rz}{p''x + q''y + r''z}\ ,\ t \cdot \frac{p'x + q'y + r'z}{p''x + q''y + r''z}\right);$$

when f is still homogeneous relatively to its three variables, but ϕ is an arbitrary function, and $p \ldots r''$ and t are ten arbitrary and independent constants. Change now (as you desire) x, y, z, to $x + a$, $y + b$, $z + c$, where a, b, c are three new arbitrary constants; and suppose these to be connected with the ten preceding constants by the three relations,

$$pa + qb + rc = 0, \quad p'a + q'b + r'c = 0, \quad p''a + q''b + r''c = t,$$

we shall thus have the new equation of transformation,

$$f(x + a, y + b, z + c)$$

$$= \phi\left(\frac{px + qy + rz}{1 + t^{-1}(p''x + q''y + r''z)}\ ,\ \frac{p'x + q'y + r'z}{1 + t^{-1}(p''x + q''y + r''z)}\right).$$

Conceive next that a, b, c, t, together tend to infinity, still satisfying the three conditions lately written, while x, y, z, and $pqrp'q'r'p''q''r''$ continue finite; the limiting result will be the following:—

$$f(x + a,\ y + b,\ z + c) = \phi(px + qy + rz,\ p'x + q'y + r'z),$$

which seems to meet your requisition, since it does not *expressly* refer to geometry, though you will not fail to see that the process was *suggested* to me by geometrical considerations, namely, by the conception of a cone reduced to a cylinder by being *thinned* while its vertex is *removed.*

'If we neglected so to *thin* the cone, we should only *magnify* it into a vast helmet in some Castle of Otranto; whereas we want to convert it into a gigantic extinguisher, fit to be clapped upon a wick *unsnuffed* for an eternity, and by consequence infinitely *long.* If you laugh at the bull, don't forget that it would be just as

much a blunder if any beginner were to fancy that (for example) the equilateral or rectangular *cone* of revolution,

$$x^2 + y^2 - z^2 = 0,$$

could ever become a *cylinder*, by the *mere remotion* of its vertex. The surface

$$x^2 + y^2 - (z + c)^2 = 0$$

is still an equilateral cone, however large the constant c may be; but if we *thin* this cone indefinitely, by (for instance) introducing the factor c^{-2} into its last term, and thus writing

$$x^2 + y^2 - c^{-2}(z + c)^2 = 0,$$

then, indeed, the limiting result will be a cylinder, namely,

$$x^2 + y^2 - 1 = 0.$$

'Lest this letter should either grow like the wick of my countryman's candle, or on the other hand be quite blown out, and fail to reach your eyes at all, by any incipient procrastination, I shall send it away at once.

'I have not been in Dublin since, to forward my batch of Abstracts.

'A Dublin paper, which some time ago accused Mr. Hargreave of being *young*, has lately been saying that Baron Richards is *here*, Doctor Longfield *there*, and Mr. Hargreave *nowhere*.* Do *you* happen to know his present address? I was very glad to meet him.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*October* 5, 1849.

'Your solution is exactly the thing—some commodious way of keeping the lower end of the cone within bounds was just what I wanted.

'I have heard from the Member for Belfast, who will support the memorial (and has supported the claim already) in favour of the disbanded professors. He thinks it will be referred by Lord John Russell to Lord Clarendon.

'I do not know Hargreave's address.

* Commissioners of the Encumbered Estates Court.

[He then returns to the question of the area of a circuit as it is discussed in the Paper in the *Cambridge and Dublin Journal of Mathematics*, already cited, and concludes] :—

'For your boys you may put it thus:

'To puzzle a churchwarden, let the black line be the boundary of the parish, then the parts shaded red are out of the parish.

'If a part marked 1 pay tithe, then (2) pays double tithe; but to the inhabitants of − 1 the parson pays tithe.

'If a boy driving his hoop in 1 is to have one cut from the beadle's cane, then a boy doing the same in (2) is to have two cuts; but a boy so employed in − 1 may give the beadle a cut, I think with his own cane, what does the formula say? May the boy use his own hoopstick?'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory of T. C. D., *October* 10, 1849.

'Many thanks for your figure, which I am sure will be worth studying. A little daughter of mine, about nine years old, who has a *formula* for anything new she learns from me, "Deeply interesting, combined with being deeply curious and instructive," after gazing for some time on your "walls of Troy"—so my schoolboy companions used to call a labyrinth on a slate or paper—commenced her usual exclamation, and got as far as "Deeply interesting, combined with being deeply curious"; but there stopped short, and said, "I cannot add, *and instructive*, for I do not understand it at all."

'Her papa is in a *little* better condition on the subject, and that is all he can say, as yet. But in earnest, from such attention as I have hitherto given to the matter, I think that the speculation is not unlikely to have even *practical* fruits; nor would it at all surprise me if some *electrical* phenomena of circuits were found to correspond to your law.

'The remarks on integration *reminded* me of a mode of proving by quaternions an important but known proposition respecting polyhedra, which corresponds to the physical possibility (or fact) of equilibrium of a closed body immersed in a fluid of the same specific gravity; but though you were one of the first persons who noticed the Quaternions, the proof might merely *bore* you.

'The *enunciations* on the first half of this sheet may, however, interest *you*, though the *Athenæum* leaves them unreported as *abstruse*.* I own I should be curious to *see* (what no doubt *can* be given) a demonstration of either theorem by co-ordinates.

'You saw, of course, what (oddly enough) I did not at once perceive myself, that I was tacitly assuming your homogeneous function f to be of dimension zero; but this seems not to limit the essential generality of the question.

'I have not yet got to Dublin to send off my parcel for you. The enclosed note will show that I was not allowed to indulge myself respecting Professor Young in the way I proposed; but I may still be of some little use, by signing a memorial.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7 Camden-street, Camden Town,
'*October* 11, 1849.

'Nothing about Quaternions will *bore* me, if I can only make it bore *through* me. Ink must be cheap in Ireland if you can afford to waste it on such a supposition as that.

'I have thought more than once about electric currents in reference to my area conundrum. I suppose the qualitative character of formulæ expressing the action of a current on a point must depend upon the law of signs I have given—in some way.

'But I am a searcher after things mental—not material—and to me, whose business of life it is to study the development of the mind—the interpretation of

$$\Sigma\,(x_m\,y_{m+1} - y_m\,x_{m+1})$$

is *practical*—while the progress of electricity through a wire is comparatively *theoretical*.

'The demonstration is very easy. I hope to have it in print in the next number of the *Cambridge and Dublin Mathematical Journal*.

'I could make a teetotum game out of it for your little girl.

'I agree with the *Athenæum*, that your theorems are abstruse; but they should have reported them for those who like abstruse

* Respecting inscription of gauche polygons in surfaces of the second order. Printed in *Proceedings* of the R. I. A., for June 25, 1849.

things. It is a case in which geometry must teach algebra a demonstration.

'Your theorem ought not to subdivide. The odd polygon ought to be the even one, with one evanescent side. Pascal's theorem about the hexagon is equally a theorem for a 5- or 4- or 3-sided figure. For instance, draw a $\triangle$ and tangents at the angular points. These tangents are the *directions* of evanescent sides of a hexagon, and the theorem of the intersections remains.

'I have not heard anything more of poor Young.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*November* 12, 1849.

'Your parcel received this day by Mr. Erck; and I await your instructions as to the disposal of the copies for which you have a disposal. The rest I will take care shall go to people who are up to such things.

'Here is an enigma for your little girl: Who ought to have his heart in his head? Answer—A teacher; for, *primo*, he ought to have his heart in his subject; *secundo*, he ought to have his subject in his head; *ergo*, he ought to have his heart in his head. The only objection I have heard is from a teacher of anatomy, who objects very much to having any heart in his subject except that subject's own, and sees difficulty in carrying his subject in his head.

'With thanks for my copy of the above-mentioned.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 6, 1850.

'I cite you to tell me who the abstracts are for, with the following theorem:—

[He then takes the equations of a line $x_1x + y_1y + 1 = 0$, and calls this line and the point P, whose co-ordinates are x_1, y_1 *relatives*. He next takes the equations of two conics such that the coefficients of the second are the same as those in the tan-

gential equation of the first, and mentions that those of the first are similarly related to the co-efficients of the second.]

'Or the connexion of the co-efficients is one of reciprocity.

'Moreover, if any point whatsoever be taken in the plane of the two curves, say P; and if the curves be called (v) and (V),

'The *polar* (point) of the *relative* (line) of P, derived from (v) is the same as the *relative* (point) of the *polar* (line) of P, derived from (V). And (v)ice (V)ersâ.

'And this last theorem contains, and is contained in, the relation of the co-efficients mentioned above.

'I was looking to day over my *Cambridge and DUBLIN Mathematical Journals*, making them up for binding. I really think it incumbent upon you to write to the editor, in the name of the Irish nation, to know what he means. Perhaps you would prefer to call a council of Irish mathematicians first, to discuss what steps should be taken.

'You will observe that, from the moment that the Dublin Journal was joined with the Cambridge one, a certain Greek motto was put on the title-page. Now under this motto lurks a sly and insidious piece of Saxon impertinence (it is well O'Connell is not alive). The motto is, Δυῶν ὀνομαιων μορφη μια—which can mean nothing but "Of our two names Murphy is one." This is too bad, really: who would have suspected a quiet man like Thomson of such a thing?'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *Tuesday night, April* 9, 1850.

'The theorem you have been so good as to communicate, in a letter received this evening, seems to me (on, as I own, too short an examination), to be included in the following, which I suppose is known:—

'"If two conics (A) and (C), be polar reciprocals of each other, with respect to another conic (B); then whatever point P may be taken in their common plane, the (A) pole of its (B) polar coincides with the (B) pole of its (C) polar."

'In fact, for three concentric circles, with radii in geometrical progression, this theorem is obvious. And I think that a person accustomed to the modern extensions of geometrical results could

scarcely doubt, after seeing this, that the theorem is generally true.

'Now let the conic (A) be your curve (v), and let the conic (B) be the imaginary unit circle round the origin,

$$x^2 + y^2 + 1 = 0;$$

the conic (C) will then become your other curve (V).

'Thus substituting (v) and (V) for (A) and (C), and observing that the (B) pole of a line is what you call its *relative point*, while the (B) polar of a point is what you call its *relative line*, your theorem emerges under the form that

'The (v) pole of the *relative line* of any assumed point P in the plane coincides with the *relative point* of the (V) polar of the same assumed point P; the conics (v) and (V) being of course (as you notice) interchangeable.

'I shall be glad to know (at your leisure) whether this view of your theorem has occurred to you, and whether you approve of it.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 14, 1850.

'Be it known unto you that I have discovered that you and the other Sir W. H. are reciprocal polars with respect to me (intellectually and morally, for the Scotch baronet is a polar bear, and you, I was going to say, are a polar gentleman, only I thought perhaps you might go and say I called you an Esquimaux). The intellectual polarity is of the kind $\phi\psi x = -x$. When I send a bit of investigation to Edinburgh, the W. H. of that ilk says I took it from him. When I send you one, you take it from me, generalize it at a glance, bestow it thus generalized upon society at large, and make me the second discoverer of a known theorem. He cuts my legs off; you make a pair of legs grow out of my head, and turn me upside down to stand upon them. His process after yours gives $\phi\psi x = -x$. Reciprocal polarity the last and most agreeable; your process involves no writing of pamphlets.

'I believe you have hit it, but I cannot find that your theorem is known. I have not yet demonstrated it; but, from your instance

and mine, it may be that this theorem is only true when in the curve of reference—

$$\text{coefficient of } y^2 = \text{coefficient of } x^2.$$

Poncelet has no theorem of the class that I can find.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 14, 1850.

'At you again. I take it that the theorem you gave me *is* involved in the very definition of reciprocal polars.

[This he establishes by stating the now well-known principles of polar reciprocation.]

'N. B.—It is not necessary that the reciprocal polars should be of the second degree. Now I very much doubt all this being found anywhere.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *April* 18, 1850.

'I abstained yesterday from reading your note *No.* 2, as intending to *turn on* the quaternions, and prove (or disprove) by them the theorem which a former letter of yours had suggested to me. The example of the concentric circles, although encouraging, did not upon consideration appear to me decisive, because, as you know, any two *such* circles in one plane are considered to have double contact at infinity. I therefore attacked, this morning, the analogous question respecting *surfaces* of the second order, using my own calculus, which adapts itself, at least with me, *more easily* to surfaces than to curves; and I soon had the satisfaction of seeing the theorem confirmed, at least for the case when the intermediate surface is a sphere, which case appears to me to be *quite general enough*, and by no means confined to yielding a mere induction or probability, however strong. In fact we may always, by real or imaginary deformation, reduce *any one* central surface of the second order to become a real sphere of a given radius round a given point as centre. Only what had before been real *may* then

become imaginary, and we may have to consider imaginary points upon the real sphere, and I infer then that, *in space*, "If any two surfaces of the second order, A and C, be reciprocal polars of each other with respect to a third surface of the same order B, the A-pole of the B-polar of any point P whatever coincides with the B-pole of the C-polar of that point." But my mathematical learning is far too slender to enable me to pronounce with any confidence that this is a new result.

'Since satisfying myself of its truth, I have read your note No. 2, which seems very clear and satisfactory. Only I do not quite understand how you propose to extend the theorem to curves beyond the second order, on account of there being then *several* tangents, and no *one* definite rectilinear polar of a point; but probably you have some way of fixing what seems here unfixed. Perhaps you use conics instead of right lines, in passing to curves of the third degree. It is fair to remark that for conics your theorem may be regarded as virtually *including* the one which it suggested to me, and not merely as being itself *included* therein; for the imaginary unit-circle $x^2 + y^2 + 1 = 0$, which I called the conic (B), may be made by deformation to represent any other conic. My merit in the matter, if any, is very small; and I shall be happy, if I ever print my result in a Magazine or Journal, to acknowledge that it was suggested by a communication from you. I wished to show you that I am not *always* so stupid, indolent, or busy, as to lay aside unstudied the hints with which you occasionally favour me; and I took the opportunity to let my eldest boy see me go through some work with co-ordinates, for the purpose of verifying your theorem, and confirming the guess that it gave rise to. As to the subsequent extension to surfaces, I shall just copy here a formula or two of such co-ordinate work; but assure you that they are (in my case) mere *translations* from a *previous*, more general, and, as I think, generality being allowed for, more simple investigation with quaternions.

'Let, then, the first two surfaces be

$$\text{(A)} \qquad a^{-2}(x-l)^2 + b^{-2}(y-m)^2 + c^{-2}(z-n)^2 = 1,$$

and

$$\text{(B)} \qquad x^2 + y^2 + z^2 = 1:$$

let also the point XYZ be the B-pole of the A-polar of any assumed

point xyz, we shall have the relations

$$\frac{a^{-2}(x-l)}{X} = \frac{b^{-2}(y-m)}{Y} = \frac{c^{-2}(z-n)}{Z}$$

$$= 1 + a^{-2}l(x-l) + b^{-2}m(y-m) + c^{-2}n(z-n)$$

$$= \therefore (1 - lX - mY - nZ)^{-1};$$

whence the equation of the reciprocal polar of (A), taken with respect to (B) is

$$(C) \qquad a^2X^2 + b^2Y^2 + c^2Z^2 = (1 - lX - mY - nZ)^2.$$

'If now we seek the B-pole, xyz, of the C-polar of an arbitrary point of space, XYZ, we are reconducted to the same relations as before; which proves the theorem, at least for the two assumed surfaces (A) and (B) above, and therefore, by deformation, for *any two* surfaces of the second order.

'Hoping to write soon again, I remain, in the meanwhile,' &c.

[A letter of Sir W. R. H., dated August 26, 1850, replies to one of De Morgan's on a logical subject. It is not here printed, as not easily intelligible without the letter to which it refers, and which I have not been able to discover.—R. P. G.]

From the SAME *to the* SAME.

[FROM A DRAFT.]

'OBSERVATORY, *October* 19, 1850.

'1. I am in the mood for writing to you something about the first principles and fundamental rules of the Quaternions considered as a *Calculus*, and that I may diminish the risk of boring you, and have a chance of extracting your comments with the least possible trouble to yourself, I shall take as my text-book your very valuable little volume on Trigonometry and Double Algebra (London, 1849), which you were so good as to send me at the time of its publication.

'2. The Calculus of Quaternions may perhaps be defined to be that extension of ordinary Algebra in which the square roots of negative numbers are interpreted as denoting vectors, or directed right lines in tridimensional space; and in which such lines are

supposed to be not generally commutative with each other as *factors* in multiplication. They may, however, be so commutative as factors, namely, when they are parallel to one common line; and thus the Calculus of Quaternions includes all ordinary Algebra.

'3. A general theorem of (ordinary) Algebra is not necessarily (nor even usually) applicable, without modification, to quaternions; but *every* general theorem of quaternions is true for ordinary Algebra. For example, it is not true generally in quaternions that

$$(r + q)^2 = r^2 + 2rq + q^2;$$

but it is true in algebra, as well as in quaternions, that

$$(r + q)^2 = r^2 + rq + qr + r^2.$$

'4. Again, it is not generally true in quaternions that

$$d \cdot q^2 = 2q \cdot dq,$$

when d is the mark of differentiation; but it is true in algebra, as in quaternions, that

$$d \cdot q^2 = q \cdot dq + dq \cdot q.$$

'5. Again, we must *not* write in quaternions, as in algebra,

$$d \cdot q^{-1} = -q^{-2}dq;$$

but we may write, in algebra, as in quaternions,

$$d \cdot q^{-1} = q^{-1}dq\, q^{-1}.$$

In general it may be said that the differential and integral calculus of quaternions remains to be formed; although a few things have been published by myself on the subject, and on certain analogues to Variations and Partial Differentials, with some applications and some solutions.

'6. Although my "vectors" are all square roots of negative numbers, I yet am in the habit of *avoiding* the use of the symbol $\sqrt{-1}$; not as at all *uninterpretable* in my system, but as being *too easily* interpretable; as being in fact (in it) *indeterminate* and *vague*. *Every unit-vector* in tridimensional space is with me *equally* entitled to be described by the symbol $\sqrt{-1}$; I therefore usually avoid to use that symbol as denoting any *one* such unit-vector; and employ, commonly, for any *particular* vector, whether equal or not equal to

unity in length, some special symbol, namely (such is my custom) some one small Greek letter, selected according to what may seem the [most] convenient in the case, and having accents or indices annexed, or not annexed, according to what seems convenient. Thus, α, β, γ, . . . α_1, β', γ'' . . . are all with me (often) used as symbols of vectors; and then it is to be understood that I regard the square of each as a negative number.'

From the SAME *to the* SAME.

'OBSERVATORY, *November* 26, 1851.

'I have often been so abominably lazy as to acknowledging your kind attentions that on this one occasion, at least, I resolve not to lose a post. Let me then thank you for your Paper, on "Recent Discoveries on the Invention of Fluxions," received by post this morning.

'I think that you do our species a service, by assisting to vindicate, from the charge of plagiarism, the memory of a great man [Leibnitz]. For my own part, I find it difficult to believe in any instance of such a thing, it is so *pleasant* to confess an obligation. Yet with the usual inconsistency of human nature I have (I own it with regret) at times suspected that *I* have been plagiarised from, by one (not very high) contemporary—not *you*, nor any one *like* you. But I deeply distrust such distrust.

'My book on the Quaternions is advancing rapidly—I have just been correcting the slip 2 F 3, which will bring it somewhat beyond 440 octavo pages. I first aimed at 200, but shall now congratulate myself if I get off under 500 pages.* You should be most welcome to copies of all the sheets hitherto printed, if I fancied that you would accept them, in the present state of the publication. In fact I should *like* to send them, but think it not quite fair to *force* what may be thought a confidence on anyone.

'You might do me a material service, if you chose, with little or no distraction to yourself. I mean that you are so *much* better acquainted than I am with the *history* of science, ancient and modern, that it would cost you little or no trouble, while it would be hard work and new work to me, to put the finger on the books which ought to be acknowledged, as preceding the invention of

* The number, including the preface, did actually reach 888.

the quaternions, and as having any sort of connexion therewith. Sometimes it has occurred to me to ask your leave to *reprint* your list of references, prefixed to your own remarkable book on Double Algebra; but again, I have thought that this might seem to be assuming to myself the merit of a certain amount of research to which I have no claim; for there are several of the works, referred to in your list, which I have never even seen. Indeed it is as notorious as public confessions of mine can have made it, that a degree of familiarity with what you have happily named Double Algebra, preceded in my own mind, by several years, the thought of quaternions and *sets;* although the conception of such "sets," suggested by views about *time*, occurred to me at least as early as 1834, and was publicly written of by me in 1835. But, on the whole, I have several times thought, and Charles Graves appeared to agree with me when I once mentioned it to him, that the relation between Double Algebra and the Quaternions is rather one of *contrast* than of resemblance, although it is undoubtedly possible, by some not difficult modifications, which I should be happy to submit to you, to exhibit the quaternions as *including* the results of Double Algebra. A quaternion may always be reduced to the *form* of a *couple*,

$$a + \sqrt{-1}\, b.$$

But in my nearly finished work, which will, however, cost me several weeks of hard work still, I have chosen, for the sake of what seemed clearness, to take a more purely *geometrical* view of the whole matter, with a continual reference to space of *three* dimensions. And it is *chiefly* with reference to *such* a view, that I desire to be furnished with references to persons who may have preceded me. But if, either on this or any other aspect of the matter, you choose to favour me with any remarks, I shall very gladly receive them; and if *you* choose, or permit, it would give me pleasure to prefix, or append, to my book, any letter of yours on the subject.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*November* 26, 1851.

'I have seen *one* sheet which your son brought me—and a very likely youth he seems—I think you will find him make his way—if one dare judge by a few minutes' conversation.

'I beg you will send me the part printed, without scruple. There is a pleasure in reading while anything that strikes may do service; it is the reviewer's feeling Christianized.

'There is no danger of my being in any of your thoughts, in any manner which may oblige me to take care what I do. For I have, ever since I published on triple algebra, had a kind of repugnance to letting the country beyond double algebra occupy my thoughts, and I am quite bare of everything except casual reading on quaternions from your pen. I hit accidentally upon what I believe to be the reason of this very curious—and with me unusual, indeed unique—recoiling from a subject I had once thought about. Taking up a copy of my Paper on Triplets I saw the date, and it reminded me that while I was thinking about it poor F. Baily was lying on his death-bed; and, as I used to walk down day after day to hear that he was growing worse and worse I had the triple algebra in my thoughts; and I believe that after the Paper was printed I used to remember it with a kind of disgust.

'Another Sir W. Hamilton once offered me communication of his unpublished speculations, and we had a row about it. But there is no fear here. First, because I am not on the same subject; secondly, because you are not a controversialist by habit, and have too many novelties in your head to be mono—something, or very near it—about one. The history of Sir W. H.'s mind on the quantification of the predicate would be a curious thing.

'Assuredly the quaternions must contain the double algebra. Your couplets of time must have suggested *triplets* and *n-lets*. (N. B.—What are $\sqrt{-1}$-lets.) But you can hardly have failed to look at the question in connexion with double algebra—*not* as time—at the moment when you thought of the triplets. I should say—*pace* Graves and yourself both—that both contrasts and analogies must have occupied your mind. In fact, we may ask, do *contrasts* ever strike except in connexion with *analogies*? We *contrast* green and red: do we ever think of the contrast between *green* and *loud*? certainly not, except when we happen to contrast sight and hearing—*perceptions* both. *Differences* are, I think, not thought of as other than the separators of *species*—under one *genus*. And there is logic for you.

'I should be very glad, if you would tell me in confidence of

whom it passes through your mind that he has plagiarised you. I think I might perhaps remove the impression, or point out something still stronger in some person above suspicion. I have thought a good deal about plagiarism—guilty and innocent. There is a kind of plagiarism which no one can guard against. It is the *light* which another person's results throw upon his own previous thoughts, without taking an atom of his wick or his oil.

'I see that by a most extraordinary forgetfulness I have not mentioned your time-paper in the list you refer to. I can only account for this by supposing that I made sure I could not forget it, and made no note, or perhaps I could think of nothing but the geometrical view.

'Wheatstone (who has a great love for pure speculation) is very strong upon what the Germans have been doing. When I get your sheets I shall apply to him to know what he can tell me.

'P. S.—Unfortunately one cannot clear Leibnitz without inculpating others. I think myself that plagiarism is very uncommon among really great men. But I have my doubts as to Laplace.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Royal Irish Academy, *December* 1, 1851.

'I received with great pleasure your note, by a late post on Saturday, when I was about to start for an evening meeting of the R. I. A., and now write these few lines to *acknowledge* it, at the Council table of the same body, after the other Members have gone.

'At this moment I only send you as an *instalment* the first two sheets, and you will see that the printing was *begun* so long ago as 1848, though it is only lately that I have been really *active* at the work.

'I am aiming at publication *early* next year, but shall prize very much any criticisms from you, and any hints respecting anticipations, or even concurrent or later investigations, which have any connexion with the subject. Something (not much) I do know of what has been done abroad, which I hope to notice in preface, notes, or appendix. My knowledge of it is too slight, and came too late, to enable me with any convenience, or indeed propriety, to speak of it in the text; it is, for instance, pretty recently

(long after my printing was begun), that I have heard anything of an employment like my own, of $B - A$ in geometry. To me the thought of using that symbol was familiar, I suppose twenty years ago. More than sixteen years ago I *published* the analogous use of it, as denoting what I called a *time-step*, as it is in the quaternions a *space-step* (from A to B). So little am I *sore* on points of that sort, though indeed it would be hard for me to have that feeling towards *you*, that I *never missed* my name, or the "Pure Time," from your list of references (which is not just now at my hand), nor do I think that you were *bound* to refer to it, even if it had been in your remembrance.

'I have every confidence in Wheatstone. Act as you think fit respecting him.

'How do you advise me to send the other sheets?'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 4, 1851.

'Being deep in such a sublunary thing as a life assurance report, I have no time at this moment to do more than acknowledge. Next week I shall set to reading—being (this word to prevent ambiguity) yours truly.'

From Sir W. R. Hamilton *to* A. De Morgan.

[from a draft.]*

'Observatory, *December* 8, 1851.

'If you allow me to continue to correspond with you on the subject of my forthcoming work, you must make up your mind to my being often intensely egotistical, especially at present. Remember that as far as *separate publication* goes, I am a *virgin author*! feel quite conscious and embarrassed, although having been in *some* degree before the mathematical world since my boyhood. It is almost exactly twenty-seven years since Dr. Brinkley laid my first paper (and an elaborate one it was; it is in my possession) on a general theory of Caustics before the Council of the Royal Irish Academy.

* It would appear that this letter was not sent.

'You would do me no great good by criticising—and I can conceive your feeling a temptation to do so—the metaphysics of the early articles *at present*. I know that I have not done justice even to my *own* views in that direction, much less to existing philosophy: *sed liberavi animam meam*. I *could not* bring myself to enter on the subject without some such introductory remarks, and must only hope that while thus imperfectly recording some moods or frames of mind, which have really influenced *myself* in mathematical speculations of the class we are now considering, I shall not have materially embarrassed the path of any student who will have even a moderate degree of patience to wait till he sees *how I use* the notions with which I profess to set out. At one time I read a good deal of Kant's works in the German, besides portions of Plato in the Greek: it is one of my hopes to resume, at what may be called leisure hours, some of my old studies of that kind, and to combine with them the reading of some other and more Aristotelian than Platonic works—including the "Formal Logic?" —although my own personal temperament of mind is far more Platonic than Aristotelian. Don't be so malicious as to quote the *Malim cum Platone errare*, however applicable you may think it to be; and let me tell you that when I was a boy at College I acquired some undergraduate renown by a short proof (which I have totally forgotten, and which would at all events have been since superseded by one of Mr. Boole's), that in no legitimate syllogism can the conclusion change place with either of the premises.

'At Oxford, in 1847, I showed as *your moon* to Mr. Hallam, the historian, reflecting on him at breakfast in Christ Church your illustration about the white cravats and white waistcoats (was that it?) having each a majority in some party, *some member* of which was thence inferred to have *worn both*. The earth revolved on its axis; and Mr. Hallam told me the next morning, at our next breakfast, that he was convinced. It is only honest to add that I passed an evening in company with my namesake in Edinburgh in 1850, and thought him an interesting person, although it was often very difficult, from his physical infirmities, to collect distinctly what he said.'

From the SAME *to the* SAME.

'OBSERVATORY, *December* 10, 1851.

'Thanks for your line of acknowledgment of my first two sheets. You have, I hope, received three others since.

'I have not *half*-answered your former note, but shall return to subjects contained in it—plagiarism and all that. As to myself, I am *sure* that I *must* have *often* reproduced things which I had read long before, without being able to identify them as belonging to other persons. However, in the present case, as regards my work in hand, *you* will see, and I must try to make it plain to *others*, that I do *not* design it as a treatise on algebra, nor on geometry, nor on the philosophy of mathematical science, nor even on the interpretation of the symbol $\sqrt{-1}$ (which, in fact, I rarely use); but simply as an introductory treatise on the quaternions, designed to make quite clear the meanings of the chief *notations* of my calculus, and the truth of the chief *principles* which I employ, with respect to the *combinations* of its peculiar *symbols;* the whole being viewed, on *this* occasion, with express and almost exclusive reference to geometry of *three* dimensions, although illustrated from astronomy, and indeed partly from mechanics. Unless it can be shown that I have been anticipated in the *conception* of the *quaternion*, especially as connected with *such* geometry, or in the *laws of combination* of my i, j, k ($i^2 = j^2 = k^2 = ijk = -1$), not to speak of the notations K, S, T, U, V, which *might* at need be dispensed with, I shall with great equanimity see claimed any subordinate or illustrative results; though it is true that I *do*, at present, suppose many of the *results* of the calculus to be new.

'Even if the *ijk* themselves should at last go overboard from my own bark into the general sea of science—a very bad metaphor! or (better) if, on farther search through my own little island, I shall find planted some former flag, I trust that I should still lose no jot of heart or hope.

> 'Non ego, cum scribo, si forte quid aptius exit,
> Quando hæc rara avis est, si quid tamen aptius exit,
> Laudari metuam: neque enim mihi cornea fibra est:
> Sed recti finemque extremumque esse recuso
> Euge tuum et Belle.

'As to my terminology, I took pains with it, but am aware

that plausible, perhaps just, objections may be made. I might have shrunk from adopting the word "scalar," which I did (I think) in 1844, if I could have known that you were to use that word in so different and in so important a sense, at the close of your Double Algebra. It was not easy to foresee that you would establish such formulæ as

$$(a +_{,,} b) \times_{,,} (c +_{,,} d) = a \times_{,,} c +_{,,} a \times_{,,} d +_{,,} b \times_{,,} c +_{,,} b \times_{,,} d ;$$

the common value here being

$$\gamma^2 \{\lambda\ (\lambda a\, \lambda b)\ \lambda\ (\lambda c\, \lambda d)\}.$$

I don't want to explain away my obligations to Double Algebra—in the filiation of ideas, it was the parent, or grand-parent, of the Quaternions—an undutiful chap, who complains that its *old dad* does not go *fast enough ahead*! In fact, as respects *geometry* at least, my expectations from Double Algebra are very limited. Even within the field of the two dimensions, it has in my eyes the fatal fault of leaving *no imaginaries*, or at least *no impossibles*: an odd complaint to make! but there *really are unrealities* in geometry, such as the intersections of a circle with a wholly external right line, and these *must* be dealt with. Now my quaternions deal with all such with ease and profit, at least my *biquaternions* do so, a biquaternion being

$$q + \sqrt{-1}\, q,$$

where $\sqrt{-1}$ is the *old* symbol, and q, q' are quaternions. They reproduce, for instance, Poncelet's *ideal sections*, and lead to many new and analogous results.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*December* 15, 1851.

'I have received up to page 112, and have pretty nearly read them, but am too busy to write this week. However, I think you need not be afraid of missing any acknowledgments you ought to make. Next week I will discuss this topic.'

From Sir W. R. Hamilton *to* Professor De Morgan.

Observatory, *December* 16, 1851.

'I intend to have a set of *notes*, at the end, with no other arrangement than the *numbering* of the *articles* of the text, so that there will be the greatest ease and freedom for inserting any remarks which you may think fit to make, and may allow me to insert, with your initials, or name in full, if to *both* of us that course shall seem good, as I can scarcely fancy *my own* objecting to it.

'I am quite aware of faults, but do not feel, or rather do not judge, that longer time spent on consideration would have made the work much less faulty. And as to *history*, I despair of writing anything worth notice on *that*, within the remaining time, except so far as I may be assisted by friends. Perhaps I shall be driven to make one sweeping and true declaration of want of preparation on that head.'

From Professor De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 17, 1851.

'I am glad you have adjourned till February. Do not be too sure even of that month.

'I will set you at rest about the history in a day or two.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 18, 1851.

'Of course you must be right in saying that the sheets of my Lectures, which you had received when you last wrote, went no farther than page 112. But I had fancied, and had even written it down as a memorandum in one of my many manuscript-books, from recollection, however, when so writing it, that my sealed packet of the 10th contained the three sheets, A, H, I, extending to page 128. It will console me for the blunder so committed in my memorandum made from recollection of a packet dispatched in haste, if I shall find myself to have *also* blundered in *another* part of the said memorandum, and *not* in my letter to yourself. I refer to my quotation from Persius—one that, like some others

from the same stiff, stoical, uncyrenaic, and non-Horatian writer, although he had the taste or sense to admire Flaccus, impressed itself long ago on my own somewhat stoical mind, such as the "Nec te quæsiveris extra," on which indeed I plead guilty to having once written a sonnet. *Was* I then *such* an ass as to write

"Euge tuum *atque* Belle"?

instead of *et*. After dashing off my letter to you, I went out to walk in my garden—which was once Brinkley's, and has had its walks trod by many illustrious visitors; Wordsworth was once a guest of mine, and I have a "Wordsworth's Walk," and the said garden led me long ago into the perpetration of a couple of other sonnets—and while I was so walking, the enormity of my offence against Prosody flashed across me, if indeed I *did* commit it, by writing the aforesaid *Atque*. What could De Morgan think of me! To be sure, he *says*, in his "Formal Logic," that he is not scholar enough to give a decided opinion on the meaning of a certain Greek verb; but that is just one of the few points on which I *won't take his own word*.

'By the way, I picked up yesterday, for the splendid sum of one shilling, a work almost exactly as old as myself, and which will be, I think, a real treat to you. I have already written in it an inscription to the author of the "Formal Logic," in the pages of which book you somewhere avow that you have been unable to procure the work I speak of; the beauty of the business being that you did not know so much as how to *refer* to it. Probably you don't even know its *name:* but *I* made sure of there being a correct and proper *title-page* (1805). 'Enjoying the pleasure of tantalising you, and of, for *once* in my life, knowing more of bibliography than yourself, I remain, &c.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 20, 1851.

'Your memorandum is right and wrong to your liking. You did send up to p. 128, and you did not write *atque* for *et*. And as to my detecting it, I should certainly have known that *atque belle* was not a dactyl, because I remember about long by position (a right the parson too often takes in the pulpit); but if it had been

one of your doubtful cases, I never could have trusted my own memory. What I say about my scholarship is but too true. In middle Latin I am a fluent reader, and that is the best I can say. At school I could do what I liked; but as I also would do what I liked, I turned off to mathematics.

'I shall be very much obliged to you for the book. I remember no place where I have mentioned a book I could not name, except in the appendix, where I say I have heard some *books* (not *book*), used in the Irish Colleges, make the rules of syllogism expressly dependent on the exclusion of contraries. Graves was my informant, but he afterwards told me that he did not so much refer to *books*, as to *cuts* (as he called them), common in college, meaning problems. But I shall be glad to see a book of your date to watch this point.

'On Monday, I think, I shall write on your question of priority. Wheatstone sent me a list of some books to day.

'And so you have not given up sonneteering. Did you ever read the poem called the "Loves of the Triangles"? or Boscovich's poem on Eclipses? or St. Prosper on Grace? the only poem the Jesuits, who educated D'Alembert, would recommend him to read; they said poetry dried up the heart. As you are a *nascitur-non-fit* poet, you have a right to your inspirations.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 23, 1851.

'I send three more sheets, and regret to say that I am not *now* sure (on reflection) that you will care much for the logical book which I picked up for you, although I still intend to forward it, as a little mark of good will. It ran in my head that you had stated, in one of the notes to the "Formal Logic," that you had heard of the existence of a work used in Dublin, without being able to procure it, which work contained some coincidence with your views, on some important point, I forget what. And I remember well that when a boy I had read carefully Walker's Logic, as we used to call it, but in fact a Commentary on a Compendium by Murray, which latter was a poor concern, and although nominally a text-book here at the time, was generally considered to have little other value than what the comment gave it. And I knew that Walker—John

Walker, founder of a religious sect, editor of Lucian and Livy, author of a sound book on the Philosophy of Arithmetic, and on the Elements of Algebra, a learned and good man, whatever *I* may think of his leaving the Church of England, which it is fair to say he did at great personal loss—had laid much stress on the possibility of treating *negative* propositions as *affirmative* ones, and of *converting* them as such. For instance, his *not-mastiffs*, *not-a-dog*, rang still in my ear. I suppose Walker conceived that the Dublin youths whom he addressed were sons of *sporting* sires, and that a talk about *greyhounds* was likely to allure the *gaudent*[*es*] *equis canibusque*. But I am by no means certain that *this* was the point (if there was *any*) on which you had been told of some concurrence with your views.

'P. S.—The edition is the *first*, and I believe now scarce—Dublin, 1805. I took care to see that there was a *title-page*, but cannot copy it now.

'2nd P. S.—I was reading for some hours, before breakfast this morning, Warren's book, and yours, on $\sqrt{-1}$, and you have narrowly escaped a letter on *that* subject.'

From the SAME *to the* SAME.

'OBSERVATORY, *December* 24, 1851.

'I am not such an inveterate sonneteer as you seem to suspect me of being, at least as to quantity, for I only composed *four* this year, and they were called forth by the death of the late Lord Northampton, with whom I had long enjoyed what might be called, and what in fact *he did* call, intimacy; while yet we were sufficiently far separated, by rank and fortune, and even by our living habitually in different parts of the world, to prevent that crushing sensation of disaster, which has hindered me from ever writing any verses on the death of any blood-relation of my own; were it not for which sensation, I might have felt inclined to attempt some elegiac record of my poet sister, Eliza Mary Hamilton, who died in my arms last May.

'I send you the only one of the three old sonnets, mentioned in a former note, which even *I* can think worth copying; and àpropos to the chief subject of our present correspondence, I throw in a sonnet on "The Tetractys," occasioned by the recollection, in

1846, of conversations with Sir John Herschel, enjoyed that year at Collingwood. On my showing the lines afterwards to a poetical and philosophical friend, since dead, Professor William Archer Butler of Dublin, he said to me, "I see clearly now that your Quaternions are a gross plagiarism from Pythagoras." A curious *acknowledgment* would not this be, to be included among those with which you and Wheatstone will supply me? I have no *depot* of sheets *here*, but send z, &c.

'I wrote something about the logic yesterday.'

From the SAME *to the* SAME.

'OBSERVATORY, *Christmas Eve*, 1851.

'To give myself a chance of forgetting the Quaternions to-morrow, I desired the messenger, who was to post my note of to-day, with the sheets Z, 2 A, 2 B, to procure, what I now enclose, and what *completes* my book so far as hitherto printed *off*, although most of sheet 2 H is in type, the three remaining sheets, X, Y, and 2 G, from the store with my printers in Dublin. I am ashamed to say, "from their *depôt*," or from my *depôt* with them, after using that word, as I now remember that I did, this morning, and omitting the circumflex accent. Certainly I make no pretension to remembering the French accents generally; an Oxonian would shudder to be told, what is too true, that I am constantly forgetting the Greek ones. But in this particular case, if I had not written in great haste, I ought to have recollected the obvious etymology, *depositum*, *dépost*, *dépôt*. I have, or had, a charming old book of fairy tales, and am sure that I have read other old French books, in which one constantly met such spellings as *estre*, *qu'il fust*, and soforth, since changed, as every one knows, to *être*, *fût*: *fête* was *feste*, &c.; though I won't assert that the rule is universal.

'My own spelling, in the book in hand, has perhaps often been capricious. I don't intend to bind myself by it in any future work, though for the moment I have aimed to be at least consistent, and even this humble aim has not been always attained, in the case of the words *far*ther and *fur*ther. I scarcely ever look into an English Dictionary, and for many years had no Latin one here—not till my boys required it; but when I was a boy myself, whatever language I was studying, I used to make it a rule, never to

grudge looking out for a word in the Lexicon, or other word-book; which of course made it more possible for me habitually to dispense with them afterwards.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 27, 1851.

'I have never fairly got time to sit down for a long letter till this evening; but I will, D. V., settle the plagiary question before I finish it. Various packets—the last received this morning—give me everything up to p. 464. To make a clear stage. First, thank you for the sonnets, the one about Tetractys is quite professional and happily worded. I trust the last line of the one to the Garden is only poetry, and that you do not set up for a really unhappy man.* There is every right to tell downright falsehoods in poetry: rhyme and reason are what my logical terminology calls sub-contraries, or as I freely construe Horace—*Semper fuit æqua potestas*—it is quite impossible to say—*quidlibet audendi*—which are the greatest liars—*pictoribus atque poetis*—painters or poets.

'The book on logic will not be a bit the less useful, because I do not refer to any specific book. If I can establish by positive evidence that the idea of contraries affecting the laws of syllogism was floating about in university problems, and by negative evidence that it was never introduced into the books, it will be a fact worth noting—one little instance of the fear of going beyond or out of Aristotle, which prevails to this day.

'John Walker was the very man to be an exception. I did not know him, but he lived in Camden-street years ago, within a door or two of where I now live. I have heard the people here speak very highly of his scholarship; and I know his arithmetic somewhat, and his geometry better. The last is a very powerful book. I shall be very glad to have his Logic, or his Notes. His books are not much on the stalls: those who have them keep them.

'As to *depot*, I hold that the word has become vernacular and has lost its accent. You say you have forgotten the Greek accents.

* 'Some transient vision, or some conflict brief
'Twixt the intruder Joy and dweller Grief.'
Supra, vol. ii., p. 15.

I never knew them, at least in school teaching. I do not believe in them. I have heard it suspected that they are the work of the time when pronunciation began to depart from quantity, and it became necessary to mark the mode of departure.

'Now as to the quaternions. Your Paper on pure time was read in November, 1833,* and the essay on quaternions ten years after (November, 1843).

'Peacock's report on analysis was published in 1834. It is so full on the foundation of algebra, and Peacock is so much master of the bibliography of that time, that I take it as beyond question the works which he omits may be considered as out of your reach, unless you have special reason to know the contrary in any case or cases.

'Wheatstone mentions the *Theoriæ Residuorum Biquadraticorum commentatio secunda*, by Gauss, Gottingen, 1832; and says that it is a notice in this work which has avowedly called the attention of all the subsequent writers in Germany to this subject.

'It seems, then, that about 1831–33 there was a *revival*, as the Americans call it, of pure algebraical speculation—that you have a share in it as the author of the Paper on pure time.

'It seems to me that all the generalities of the Paper on pure time are in the fullest sense incapable of being transferred elsewhere, unless you can do it yourself. The subject was not at that time in vogue. Mr. Warren's books (whether you saw them or not) do not treat of general interpretation, or *à priori* search after significance, but specifically treat of one aspect of $\sqrt{-1}$, and expressly reject a part of algebra as not interpretable ($\epsilon^{\theta\sqrt{-1}}$, for instance).

'I believe this Paper on pure time puts you beyond the necessity of looking elsewhere upon the whole general idea of algebra. The allusion to *triplets* in the last sentence shows you prepared, in any system which masters $a+b\sqrt{-1}$, to ask after $ai+bj+ck$. I should think that some of the fundamental points may have struck others—it is so in everything. In my first memoir on the foundation of algebra I mention those who think of AB "more of B attained by motion from A than of the quantity of length in AB." I then refer your Paper on pure time to this mode of thought,

* A mistake. It was read June 22, 1835.

from which I suppose I could then have pointed out similar modes of thought in others; but I do not remember them now; I suspect the pure time swallowed them up.

'This notion becomes very conspicuous in the first sheets of the book now in hand, in the definition of $A-B$, as distinguished from $OA-OB$. Quere, might not $(A-B)O$ be written for the second; and in $(A-B)O=\overline{AO}-\overline{BO}$—$A$ B O being points, and $\overline{AO}$ $\overline{BO}$ lengths—is there not a capacity of interpretation?

'The special subject-matter of quaternions certainly owes something to the now established theory of $\sqrt{-1}$, of which I take it to be the legitimate extension. It does not follow that the theory of $\sqrt{-1}$ suggested it to you, because the interpretations in the Paper on pure time may have done it. On this you know best.

'One very leading feature of the quaternions, and one which takes the *discovery* out of the class of extensions commonly so called, is the non-permanence of *all* the rules of algebra—as of $AB=BA$. I say takes the *discovery*—not the thing discovered—but the process of discovering.

'For one, I long contemplated something like

$$Ai+Bj+Ck=A'i+B'j+C'k$$

giving, *per se*,

$$A=A',\ B=B',\ C=C';$$

but I did not dream of ij differing from ji.

'I must now stop; but I shall go on by next post. I want to look at some old letters of yours before I say anything more.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,

'*December* 31, 1851.

'I hope you received my letter. I had intended to follow it up immediately, but the affairs of a relative intervened and took me off all work. These, however, are settled, and I hope to sit down to write further to-morrow.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 3, 1852.

'I did receive your letter, and thought it only too favourable to myself. As to $\sqrt{-1}$, it had haunted me long, and I did know the outlines of double algebra, not only before I thought of the quaternions, but also (I *think*) before I had formed, partly with the help of Kant, any very definite views about pure time. Still, you conceive, that *when* those views, rightly or wrongly, were formed, I was naturally led to see that *any* number of independent progressions might be imagined, as easily as *two*, and thus formed early that notion of "triplets and sets, of moments, steps, and numbers," which you refer to, as mentioned near the end of my time Paper. (I quote just now from memory.) At least as long ago as 1834, I formed plans for several different triplet systems, and even contrived several geometrical constructions, but was not satisfied with any of them. If you have preserved a letter of mine, which I wrote on receiving your first Paper on the foundation of algebra—or on some connected occasion—it is my impression, but I don't venture to assert it, that you will find some statement to the effect, that *if* my views about time were correct (and in *some* degree they have since been modified, so as to admit more of the *symbolic* element), there *must* be triplets, and even "polyplets." Certainly this last word was in my *head*, but I can't say whether I ventured on *writing* it to you. After a while, I may recover among my manuscripts memoranda of some of those old speculations of mine about *sets*. I know that I did *not assume*, in them, the *commutative* principle of multiplication as *necessary*, although I aimed at it. And although I was induced to print (in perhaps the Supplement to the December number of the *Philosophical Magazine*, for 1844) a hasty letter of mine to John Graves, written in October, 1843, just after the *ijk* were found, and professing to give an account of the process through which I had gone on the preceding day, yet I have memoranda which show that I had been recently reading my own Paper on algebra, and seeking to illustrate its first principles to my boys, especially as related to equal and to successive *steps in time*. So that although, at the last moment, it was *geometry* that *moulded* and *fixed* my conception of the *ijk*, I had been *prepared* for accepting it, by recent as well as by other specu-

lation, of a more *abstract* sort. By the way, you have perhaps never heard that the part of the *postscript* to my letter of October 17, 1843, which was afterwards *suppressed* in the printing, was a *permission*, "you may show," not a *commission*, "please to show," "this letter to De Morgan." You are most welcome to regard this fact, which I can prove (although I do not *blame* John Graves for using his discretion as he did, perhaps fearing to *pester* you at the time), as evidence, or indication, that your writings had influenced my thoughts, although, at this moment, I do not call to mind any one distinct suggestion which I had not foreseen. Perhaps, when I read again the early Papers on the Foundation of Algebra, I may come to remember something.'

From A. De Morgan *to* Sir William Hamilton.

'7, Camden-street, Camden Town,
'*January* 4, 1852.

'At last I get an evening to resume my talk upon quaternions. I had got so far, I think, as to tell you all I knew of the possibility of any one supposing you were directly indebted to the algebraists up to 1833. Since I wrote I have been trying to master the quaternions as you now present them, and I find it no child's play. I do not think I can venture any criticism upon the whole matter: one or two points on which I thought to raise something have broken down on further reading.

'With regard to your main question, however, I am clear that, so far as you have gone beyond double algebra, whether in the way of extension or of new introduction, you need not fear having anything which you ought to acknowledge. You are, I have no doubt, under general obligations to some of those who thought about first principles in the period anterior to 1844; but it is in that general way in which you are indebted to all the authors of your early reading. The exhaustion of the difficulties of $a + b\sqrt{-1}$ suggested to you, in the Paper of pure time, the idea of "triplets." All that is peculiar to quaternions is, I have no doubt, beyond the reach of anyone except yourself to claim. It is not yet time to decide upon the relation of quaternions to triple algebra: from your earlier publications I thought the connexion more visible than I am now clear about.

'As to double algebra, there is *a* double algebra in the Pure Time, which, as far as ideas and leading thoughts go, may very likely put you beyond much obligation even to that.

'Were I you, I should give in the preface the best account I could of the history of my own mind on the subject; and should invite any persons who could furnish you with coincidences of thought to forward their references. I feel satisfied you will be let quite alone.

'This is a short summary of a good deal of inquiry; but you know that the plea of *nil debet*, when the only one, would defy the most verbose draughtsman to make it very long.

'I have looked at the Paper of Gauss, which Wheatstone says the Germans, with one voice, cite as turning their attention to the subject. Nothing is there which can in any way suggest anything analogous to quaternions—or even to double algebra.

'I am acquainted with Ohm's work on algebra, translated by Ellis; and I am sure you are safe from him. A man who founds algebra on the *seven* independent operations of *addition*, *subtraction*, *multiplication*, *division*, involution, evolution, and taking of logarithms, is yet far from the root. I have alluded to this in my Double Algebra, p. 166.

'You referred to your use of the term *scalar*. I hold this and others to be *moveable* terms; *modulus* is another; *co-efficient ought* to be one.

'Now as to what is of more consequence than the priority question, owing to the manner in which I have no doubt that question will drop dead. I cannot too strongly press upon you to do something in the way of bare enumeration of significations, for the use of those who have been thinking over the general ground of symbols. Your multiplied explanations, essential as they may be to learners, will drive the initiated to absolute despair.

'A dozen pages of bare definition, with the curb held hard upon the illustrative power, be the difficulty to you what it may, will put the proficient algebraist in a disposition to read your developed instructions with the sense of profit. If you do not do this, he will be looking among the things he is up to at a glance for the points which he is to stop and think upon—the most unsatisfactory task I know. But, with a prefix or appendix of sum-

mary, he will be let into the pith of the matter without suspense or the chance of missing essentials in turning over pages of comparatively matter of course.

'I was frequently at a loss in what I wanted in the earlier pages for want of a complete exposition of $a+bi+cj+dk$, which comes late; and even now I am in want of a *tableau* of the whole system.

'References will be wanted to what you have done in R. I. A., in *Philosophical Magazine*, in *Cambridge and Dublin Journal*.

'I have not fairly finished the proofs yet, but I am made up upon the priority point, and shall not touch it again unless you have something to ask.

'I remember a remark of yours, that the want of impossible quantities is a defect in double algebra. But the defect is the defect of symbols. All those which result from algebra or arithmetic are fully explained. But instead of weeping for more worlds to conquer, we make them. You surely do not intend any imaginary quaternions to remain for ever. However here is the end of the sheet; so, for the present, I await some more print from you; and, wishing you a happy leap year, am,' &c.

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 5, 1852.

'The note on the other half-sheet, which I wish you to return, will show that, besides Christmas or New Year Trees, I, like yourself, have had some other business than the quaternions lately on hand. But I accomplished my purpose of signing for press the 30th sheet of my book before the old year was out.

'I never *heard* of the book you cite, but shall be very glad to consult it. Please forward with that view the enclosed line to Barthes and Lowell.

'Yours, in some haste.

'All compliments of the season.'

From the Same *to the* Same.

'Observatory, *January* 6, 1852.

'I must say a few words more about your letter of December 27th, and my own notices of it. The letter arrived, along with

several others (from other correspondents or men of business), just when I was obliged, or at least wished much, to wind up some pieces of business before the end of 1851; one of them being a series of answers to the Queen's Commissioners, with respect to my Royal Astronomership of Ireland, including a brief report of the general nature of considerably more than forty thousand observations made here since my appointment; another business being the completion of the 30th sheet of my Lectures. I do not know whether you will let me count it under the same head of *business*—at least, though pleasure, it assisted to distract me—that I was to bring my three children, on the last night of the year, to a sort of Christmas festivity at the house of my neighbour and namesake, James Hans Hamilton (M. P. for the county), where I pulled the little sweatmeat from a tree for your unknown young friend, and presented my own little daughter to Lady Clarendon.

'I must say that I enjoyed the evening at Abbotstown; although *I* was very well inclined to come away at midnight, till over-persuaded by your grave-looking acquaintance, my eldest boy, William Edwin, who thought we *might* afford another hour or two, in which opinion I could see that my little Helen joined, and even an intermediate brother, Archibald Henry. But what is all this rigmarole *about*? Why, simply that I rather *skimmed* than *read* your letter, at the time of my receiving it, and merely remembered its candid, and, indeed, generous *tone* about the pure time, &c., when I wrote my first acknowledgment of it. As to the reference to Gauss, I must presume that in my haste I did not even *see* it at first. Still my eye may have just fallen on the *name*, and then passed on, from a scarcely expressed feeling at the moment, that I was quite safe *there*. In fact, with all my very high admiration, before now publicly expressed, for Gauss, I have some *private* reasons for believing, I might say *knowing*, that he did not anticipate the quaternions. In fact, if I don't forget the year, I met a particular friend, and (as I was told) pupil of Gauss, Baron von Walterhausen (about whom I have a pleasant and not ill-natured anecdote to tell you), at the second Cambridge Meeting of the British Association in 1845, just after Herschel had spoken of my quaternions and your triple algebra, in his speech from the throne. The said Baron soon afterwards called on me here, and I gave him some printed papers, about which he said (or rather wrote) to me that

he intended to study them when he should have a little "peace." Well, *he* informed me that his friend and (in one sense) master, Gauss, had long *wished* to frame a sort of *triple* algebra; but that his notion had been, that the *third* dimension of space was to be symbolically denoted by some *new transcendental, as imaginary,* with respect to $\sqrt{-1}$, *as that* was with respect to 1. Now you see, as I saw then, that this was in *fundamental contradiction* to my plan of treating *all dimensions of space* with *absolute impartiality* [Tros Tyriusque], *no one more real than another.* Consequently I have ever since held it as a certain and established *matter of fact,* that the great Gauss and myself have been on totally different *tacks,* as regards this sort of geometry. And I have *very little* studied the theory of numbers—so little, that although I remarked to Herschel at Collingwood, in 1846, that Gauss, in his *early* work, brings in $\sqrt{-1}$, I have not even a copy of that former work, and had never so much as *heard* of the later one, till,' on a reperusal of your letter, I saw Wheatstone's reference to it. I am very curious to know *what* those biquadratic residues *are;* but feel very sure that they are *not* the quaternions.'

From the SAME *to the* SAME.

'OBSERVATORY, DUBLIN, *January* 7, 1852.

'Many thanks for your important letter, received this morning. It shall be attended to; but I was up all last night for the eclipse.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*January* 7, 1852.

'Your two received. As to the sugar-plum, small chance of its ever finding its way into a child's mouth, for my people voted it should be kept to prove to what great mathematicians could descend; it being their fixed idea—in spite of my having played at blindman's buff within these ten days—that such cattle are always thinking of their altitudes, and obscurities, and prolixities.

'As to the books, I do not mean to execute your commission until I have tried another plan. It is all very fine to send a

foreign bookseller to get the books dead or alive—that is second-hand or firsthand—and to give him a hint that you think it possible they may cost £10, a hint which his conscience may be easy in halving, when an untempted bookseller could not do more than quarter it; but it is very clear you do not contemplate forming a large library.

'I am going down to Maynard's to-day, the little secondhand mathematical bookseller, in Earl's Court, Leicester-square, who may have both books—if so, good. If not, my friend Galloway's sale comes on in February (of which you shall have a catalogue—and it is well worth looking at), where there is the French translation of Gauss's *Disquisitiones Arithemeticae*, and a volume containing ten of his tracts, which very likely has the one you want.

'You know of course that J. Graves has become a great book collector.

'You will have more of me in a day or two.

'*January* 8.—I went to Maynard's—no success. I have written to Sotheby to send you a catalogue of Galloway's sale. I think you had better wait for it, for it is a mere chance that Lowell & Co. can furnish Gauss's *Disquisitiones Arithmeticae*, which is quite out of print and scarce; probably you will get it for about a pound at the sale.

'Your Christmas party may count as business—amusement is very hard work, when it has to be done in a set time and manner. Your statement of the fact that your children made you stay longer than you intended is one I could confirm by the like. Shall we publish the joint discovery that there is no getting young people away from Christmas parties? Though you communicated it first, I can prove that I knew it. But I could publish a more wonderful thing which you could not have guessed. At a little party at my house, on Christmas Eve, was a pretty young French girl of eighteen, the daughter of people of condition—brought up in France (Paris)—not devotees, nor the least inclined that way—of wealthy connexions and wealthy herself—well educated—*who never had seen two or more people dance together, anywhere—until she saw the young people dance on the evening in question at my house.*

'Your sheet of abstract arrived with the letter. I see you find that Walker thinks.

'I will send you a short account of Gauss in a few days. I mean of the quadratic residues.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 8, 1852.

'Your letter of the 4th, which only reached me yesterday, was very satisfactory on the point of priority. I could only acknowledge its arrival, and even now can scarcely do more than thank you for the *advice* which it contains, and of which I shall not lose sight. It is clear that I *must* write some condensed summary of the contents of so diffuse a volume, or it will be thrown aside unread by the busy and advanced, even if it should not deter mere learners, although the liberality of the heads of this university will enable me to offer the book at a moderate price, not yet fixed, but to be low. You cannot think it flattery, if I say that its style was not designed for readers of *your* calibre, although I shall be very glad if you can find time and *patience* to read it notwithstanding. In fact you may observe that although *alluding* often to double algebra, I do not *assume* any knowledge of it, and indeed assume usually *very little* knowledge of mathematics, at least in the earlier and larger part of the volume, so far as hitherto printed, although I am getting just now a little deeper into ellipsoids, and shall give a touch of differentials, integrals, variations, &c., in the eighth and closing lecture, which I have nearly reached. The first was put in type so long ago as 1848, and a great part of it was designed to be, nearly word for word, what had been actually delivered in my lecture room in the June of that year, Mr. Salmon of Dublin, and Mr. Cayley of Cambridge, doing me that day the honour of attending. Distractions occurred; and, when I resumed the task of composition, it seemed convenient to continue something of the lecture *style*, although not binding myself to a mere *report* of spoken matter. I trust that *ordinary* readers will not lose by this, though I can well conceive *your* finding *much* of the book tedious. It may be fair to myself to mention, that while no one but myself is answerable for any detail, both Graves at starting, and Herschel then and since, urged me *not* to spare explanation, and not to fear diffuseness. Still I own that I *have* been too diffuse, but it may not be yet too late, especially as money is not here a difficulty, to attach not merely a copious table of contents, in which I have already made some progress, but also a concise and readable preface,

or (so to speak) preliminary essay, into which something of *history* may without impropriety enter.

'As to *you*, perhaps you may find it enough to read the seventh and eighth lectures (if time and patience allow), merely turning back occasionally to some former articles as referred to. I hope shortly to write again.

'Can you assist me to procure Mourey, Paris, 1828?

From the SAME *to the* SAME.

'OBSERVATORY, *January* 10, 1852.

'I did not intend to write to you so very soon again, for I don't usually *overwhelm* my correspondents at such a rate as I have lately been doing in your case; but just as a messenger was ordered round, to take to the Chief Secretary's Lodge, in the Phœnix Park, a note from Lady Hamilton, declining in her name and my own an invitation to a ball next week—one can't always keep up that sort of ball—arrived your pleasant letter, with the account of the young French girl, who had never even *seen* people dance; which, by the way, is all that I have done since I was *very* young indeed. Still, I do like to *see* a dance now and then, especially when *children* enter; and you must blame yourself if your last letter shall provoke me to send you soon a copy of an old sonnet, suggested by the sight of a boy asleep after a viceregal ball, chiefly for children, in Lord Anglesey's time, at least twenty years ago, and recalled to my recollection by the little festivity at Abbotstown, in this neighbourhood, lately. Meanwhile I remain, with all compliments of the season, very sincerely yours,' &c.

'About the books; the theory of numbers is not *much* in my way, as yet at least; and I shall be quite content for the moment to get even such a general notion of Gauss's quadratic residues—or biquadratic, if they be so—as your leisure may allow you to give. So I quite approve of your not sending my note to Barthes and Lowell, but shall probably buy a few of Galloway's books.

'Did you look at my solution by quaternions of the well-known topical problem, first solved by Apollonius, as I find that Eutocius records, and used by Wm. Thomson for electricity? (See Lecture vii., article 459.) It seems well fitted to exemplify some processes of calculation.

'I confess myself curious to know to what extent you have examined into the *working* of quaternions as a *calculus*, as distinguished from their *principles* as a *speculation*.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*January* 10, 1852.

'I can lend you Mourey; as you ought to see, with this letter. You will take care of it I know; and you may return it at leisure.

'Mourey would have been very remarkable if Warren had not appeared in the same year.

'I do not think you too diffuse; on the contrary, you are quite right to explain at great length; but I want a synopsis for those who are already in the subject.

'By and bye, when the French—tardily—begin to cultivate algebra as a science, they will declare that Mourey did it all. So I would not on any account lose Mourey.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 13, 1852.

'Thanks for your promise to lend me Mourey, of which I shall take every care when it arrives; and the post to this place appears to be safe, though slow. I heard of it a good while ago, and shall be very glad to see it, though I fancy the book to be little else than double algebra. If the French want an anticipation, though not a very *complimentary* one! of the quaternions [see the word "absurdes"], I can point out what might with some plausibility be claimed by them as such, and what I think will startle you, as it did me, when I met it (a few years ago, but long after my own views and notations were formed and published), on the occasion of my seeking to verify some of Peacock's references to the *Annales de Mathématiques*, which I consulted in the Library of T. C. D.

'Did not Servois show, as usual, great sagacity? At the time I refer to, I spent several hours, on a few successive days (at some inconvenience to myself, not worth explaining), in examining what was done by Argand and Français, near the beginning of the century. Argand (if I remember) was admitted to have priority of Français, and it was claimed that he had published as

early as, or at least independently of Buée, whom I never could comprehend, though he has some happy hits, such as "adjective." Cauchy adopted this from Buée, and seems to have been long familiar (from others) with the notion of Double Algebra.

"*Copy of my rude Memoranda.*

"SERVOIS.

"[Page 235, 4th tome? I forget, and cannot at present verify.]

"LAFERE, *November* 23, 1813.

"... trinomial form ...

$$\begin{aligned}&(p\cos\alpha + q\cos\beta + r\cos\gamma)\\ &\times(p'\cos\alpha + q'\cos\beta + r'\cos\gamma)\\ &= \cos^2\alpha + \cos^2\beta + \cos^2\gamma = 1.\end{aligned}$$

Les valeurs de $pqrp'q'r'$ qui satisferaient à cette condition seraient *absurdes:* mais seraient elles imaginaires? [c. a. d. reducibles] à la forme generale, $A + B\sqrt{-1}$? Voilà une question d'analyse fort singulière, que je soumets à vos lumières."

'Nothing seems to have come out of this hint ... My impression is that Servois was dissatisfied with the double algebra of Argand and Français, and threw out (with, I must say, great sagacity) the foregoing suggestion, to show what other sort of analysis would be required for geometry of three dimensions. You see that *I* solve his problem by

$$p = i,\ q = j,\ r = k,\ p' = -i,\ q' = -j,\ r' = -k.$$

'OBSERVATORY, *Jannary* 14, 1852.

'P. S.—The Mourey has arrived, and shall be taken every care of. *In a sense,* I have already read it through, but must reconsider the proof of the existence of a root. I see that either *you or* I—but I hope it will be you—must write, some time or other, a history of $\sqrt{-1}$.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*January* 15, 1852.

'I have several points to write about; but am pressed just now.

'I do not hold

$$\begin{aligned}&(p\cos\alpha+q\cos\beta+r\cos\gamma)\\&\times(p'\cos\alpha+q'\cos\beta+r'\cos\gamma)\\&=\cos^2\alpha+\cos^2\beta+\cos^2\gamma\end{aligned}$$

to be utterly out of algebra. If you attach a condition—namely, that p', &c., shall be finite—it is so; but so is

$$x=x+1,$$

and

$$x=2x.$$

I suspect something of this kind:

$$p\cos\alpha+p\,.\sqrt{-1}\cos\beta+r\cos\gamma,$$

$$\frac{1}{p}\cos\alpha-\frac{1}{p}\sqrt{-1}\cos\beta+\frac{1}{r}\cos\gamma,$$

where p and r are zeros—*related zeros*..

'The whole of this subject wants considering. To wit, the case of

$$A=B=C,$$

where A is not $=C$, because the conditions of the problem impose the form $\frac{0}{0}$ upon B.

'Mourey is *nothing* else but a *part* of double algebra.'

From the SAME *to the* SAME.

'7, CAMDDN-STREET, CAMDEN TOWN,
'*January* 18, 1852.

'All yours received; and I shall answer piecemeal.

'The sonnets are very pretty. I will back a baby against a quaternion as a sonnet subject. Now for my mechanical, rule and compass, criticism—take the following:—

'*On the son of an old friend*—"*in* a light of love"—not "*with*"; "passèd," accent not pleasant; many a year gone by? Last four lines—idea not *flashing* enough—"*from earliest friends:*" meaning not very clear without looking at the heading; whereas the sonnet should not require a heading, "*yet*"—this rather suggests "$-A$ now, $+A$ to come" than "0 now, and $+A$ to come." "To thee one day," or the like, or "some day."*

'*On a child asleep*, &c.—Line 1, *then*—a little too conversational. If the first line could be recast, *thee* might be ousted—it comes again too soon; *delegated*—a trifle too political, if a substitute for the idea could be found: "Hero-viceroy's" might come in, *perhaps* (only perhaps). All the rest perfect, with one change; transpose the last two lines; the last but one is now a parenthesis; the story not sufficiently well-known to give it the reader at "descending."†

'Speaking of what is in common algebra yet to be investigated as to the functions of $\frac{0}{0}$, &c., I could not remember when I wrote before an instance which had struck me; but I have it now.

'If there be a problem essentially of the *first* degree it is this: To draw a straight line through the two intersections of two pairs,

$$1.\ ax + by + c = 0.$$
$$2.\ a'x + b'y + c' = 0.$$
$$3.\ px + qy + r = 0.$$
$$4.\ p'x + q'y + r' = 0.$$

To do this we must make

$$ax + by + c + m\,(a'x + b'y + c') = 0$$

identical with

$$px + qy + r + n\,(p'x + q'y + r') = 0,$$

or

$$\frac{a + ma'}{p + np'} = \frac{b + mb'}{q + nq'} = \frac{c + mc'}{r + nr'},$$

and the reduced equations are of the *second* degree.

'But, choosing which we please for a term of comparison, say,

$$\frac{b + mb'}{q + nq'},$$

* [Compare vol. ii. p. 636].

† [Compare vol. i. p. 511].

we have for solutions—

1. The effective solution, giving (5).

2. $$m = -\frac{b}{b'} \quad n = -\frac{q}{q'},$$

giving

$$\frac{a+ma'}{p+np'} = \frac{0}{0} = \frac{c+mc'}{r+nr'},$$

but *not*

$$\frac{a+ma'}{p+np'} = \frac{c+mc'}{r+nr'}.$$

'Take another term of comparison, and we get the same shift in another form.

'Your MS. index is nearly what I want—as to what it goes to, quite, perhaps; but the *quaternions* would require more length—rather perhaps would get it without warning.

'It will certainly enable anyone to make a first approximation to what he might pass over as understood, so as to go *in medius res* at once.

'As to a history of $\sqrt{-1}$, it would be no small job, to do it well from the Hindoos downwards.

'I must stop now; but I have a matter for a whole sheet—which I cannot do now.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 21, 1852.

'I have just copied, from memory, two sonnets to Sir J. F. W. Herschel, written on the occasion of his return from the Cape, when a dinner was given to him at the Freemason's Hall, in London, which I, who visit the metropolis very rarely, went thither on purpose to attend. Herschel appeared to me to personify the combination of science and imagination, and you will see that my lines are an attempt to express that thought in verse.

'If, among your many and deep researches, you have made psychology, as a sort of branch of natural history, one of them, you may feel some little interest in the following problem, which has often puzzled myself.

'Among the persons who know anything about my existence and my writings, I suppose that the majority would admit me to

be a mathematician; while all, or nearly all, would say that I could only be regarded as a poet by courtesy.

'Does it not seem then to contradict one of the very tritest sayings about human nature, that I care little, or not at all, about criticisms upon my poetry, such as it is, while I own myself to be actually sensitive on the score of my mathematics?

'Wordsworth did me the honour to cut up, in a more slashing style than yours, some of my early poems. I think that I was less flattered than indifferent—although I did most highly prize the advantage of an intimacy with him.

'Slash away at my sonnets; but spare me, if you honestly can, a little praise for the quaternions; or, what will be far better, allow me the honour of *assisting* you to *use* them as a calculus; for such they certainly *are*.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*January* 22, 1852.

'All yours received, giving proof up to p. 496; and also the MSS. on geodetic lines, which I must wait a few days before I thoroughly read, seeing that I have the Astronomical Society annual report to get up. By the way, we never hear a word about Dublin Observatory. I have always supposed that the Astronomer Royal for Ireland was engaged about things of more moment, which he could do, while any decent honest man could observe. But you speak of masses of observations: could you not give us a few descriptive words for me to put into the report above mentioned. You are one of our Fellows, I see by the list. If you affirm, send to me before the end of the month.

'N. B.—The words are the Council's, though you may furnish them.

'I write now, however, to get a subject finished on which I have been thinking for some weeks—quoad quaternions and priority. I feel as if a dissertation were proceeding from me.

'The mathematicians at home and abroad are getting into a somewhat fidgetty and excitable state about priority. For a great many years I have noticed a somewhat augmenting tendency to guard themselves against others, or others against

themselves; a frequency of *reclamation*, as the French call it, or a studied tone of renunciation, when the rights of another were to be saved. Some of the fraternity, but not the most prominent, carry this a great way; their εὑρηκα always has ἐγω expressed before it, instead of understood; and, if it were not for the Greek, I should say they put the word in *italics*.

'Now, according to my notion, a person should just do his best to tell the truth about himself and others, and leave the rest to providence. There is no more use in trying to guard against accidents or disputes than in trying to word a letter. Try ever so much, and the phrase which produces more correspondence will be sure to be one of those which you never thought about.

'This is the course I recommend to you, who want some advice very much just now. I was rather surprised, all through your letters, at the unquiet tone which you held about the rights of others, and the unsettled feeling about your own. But as this was probably matter of temperament, I merely thought about the question, as concerned yourself and others, and never about your feeling on the subject, except as a thing which was to be all set right by sufficient assurance on the immediate cause of it.

'But in your letter of the 6th, when I read it over again, I was startled by your reference to my "candid and indeed generous tone." Candid and generous! thinks I—I hope not. I was writing quite judicially; and if I were either the one or the other, it must have been at somebody's expense. But then it occurred to me that it might be with reference to *myself* you were speaking, and this seemed so strange that I set myself to look back through all your letters, to see if I could confirm this. And sure enough, on January 3, I found you telling me that in a postscript suppressed in printing, you tell J. Graves, not *commissively*, "please to show," but *permissively*, "you may show," this letter to Professor De Morgan. And further, that though you do not *blame* Graves for his use of his discretion, yet, that I am welcome to regard this fact—which you can prove—as evidence that my writings had influenced your thoughts. All this rather puzzled me—I had never heard of any postscript; and, if I had, should never have regarded your willingness that I should see a certain letter as proving that anything I had written had either been seen or not—been used or not—by you.

'Again, as far back as when you sent me the proof of the last page of your R. I. A. memoir on quaternions, I find that you rather deprecate the possibility of being supposed to have done me some wrong—you *hope I shall be satisfied* with what is then said of my triple algebra memoir.

'From these things—and other little ones of the like kind—I began to see that—whether you know it or not—*I* am one of the persons about whom you are anxious as to what they may say of your acknowledgments, or failure of acknowledgments. And perhaps you rather wished to pick out of me what I thought of myself. So that, whereas the Scotch Sir W. Hamilton corresponded with me (perhaps) to make it certain that he might catch me, if I deserved it, in pillaging him, the Irishman of the same name is trying to lay a trap for himself. And further, whereas the Scotchman, being seized by a fit of illness, did actually work himself up on his sick-bed to believe I had pillaged him, and employed his returning health in raising hue and cry; so it is much to be feared that the Irishman, if he should have an attack of fever, would persuade himself that he has been purloining from me, and would only get out of bed to deliver himself up to justice, on which I should say that, though the Irishman has a better moral and social position than the Scotchman, yet, as far as delusion would go, $6 = \frac{12}{2}$.

'Now to the point. There are two kinds of obligation which one writer may have to another—general and special. The former is their joint work. A comes to the writings of B in a state of preparation which makes the writings of the latter suggestive, and which, in fact, lays the match to a train already prepared. The ideas which may start up in the mind of A may be such as none but B could excite, and yet such as B could excite in no mind but that of A. In this case no acknowledgment is or can be due, beyond, at the most, an inclusion of B, on the part of A, among the writers by whom he has been benefited, *if* he make *general* acknowledgments.

'Special obligation is incurred when A takes from B, specifically, that which another could take—the *ipsum corpus* of his thought, process, or work. Of course it is indispensable to acknowledge it.

'One reason why general obligation need not be necessarily acknowledged is, that it need not be necessarily known. Nor can any person be sure he *does* know all his obligations.

'The chances are strong that a person who feels general obligation, and knows exactly to what, has a special obligation. The chances are as strong that if he is wholly ignorant of special obligation, he is as ignorant of the precise character of his general obligation. But the most probable thing may not be true.

'Among the morbid feelings on the subject very often may be seen the tendency to imagine that others have *general* obligations. A thinks that, though he cannot exactly lay his finger on B, yet B has been at him.

'And sometimes there is an anxiety about general obligation—a fear of having incurred it—and no very clear view as to the difference between it and that of the special character. And among the ascertained facts relative to this phase of the complaint, I believe it must be set down that being Astronomer Royal for Ireland is not an infallible preservative.

'With regard to myself, you owe me nothing special—that I will be certain of. If, to use your own phrase, anything I have written has *influenced your thoughts*, there can be no ground for supposing that you can do me any wrong by appropriating the results, you not knowing of any specific influence. As to general influence, it must have been exerted by hundreds upon any one man.

'Had the double algebra never come into the world, neither would quaternions; but in what proportions the share of symbolic algebra has been contributed by Peacock, Warren, Servois, your own self (in the Pure Time), my own self, if you like, &c. &c., or any others, I do not believe you can tell, or anyone for you. A hundred years hence, some one looking down upon us all may see more of that matter than we do.

'In the meanwhile, everyone owes it to history not to push formal acknowledgment beyond its due limits, for the sake of being safe in courtesy or safe in honour. It is, as far as it goes, an untruth told in the history of the mind, to say that a debt has been incurred which has not. And those who attend to history know that mischief may thereby be done. The kind of expectation which I observe to exist, of being *mentioned*, makes it difficult

for investigators to be quite accurate. How many times I have seen a person take up a new memoir, run it over, put it down, and say, without a blush, "I just wanted to see if he mentioned my ——."

'See whether out of all this you cannot get rid of all anxiety on the matter. If you can, you will just say what you know, and be content with that. If there should be any in the world who will look for more than they get, depend upon it they are not so good judges as yourself; and, as to general obligation, we must wait a century to find a better.

'In a few days I hope to read again about the quaternions.

'My wife admires your sonnets very much, both as to form and matter, *i. e.* babies. If you can make any more, particularly on the same subject, pray send me one for her album.

'I have not forgot the promise about Gauss.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 26, 1852.

'Your long and interesting letter of last week reached me by a sort of private hand this morning. At this moment, having barely had time to *read* the letter, I can only say that it deserves to be considered, replied to, and acted on. Thank Heaven, I consider my *present* Course of Lectures as finished, and time was it that it should, for it had of late perhaps been making me nervous and fidgetty, notwithstanding the important circumstance that the liberality of the Board of T. C. D. has taken it out of the region of commercial speculation—and I am not very sensitive to criticism—certainly not at all so to *yours;* although I might not like to be sneered at by an uninformed or uncandid person, where I *knew* that great labour had been expended, and *believed* that valuable results had been attained. It is the *promise* of future service to science rather than the actual *achievement* so far, which I value in the quaternions; but I must assert my belief that, even in their present infant state, they constitute a new and powerful calculus for the solution of geometrical, and, therefore, also of physical problems.

'You will see that I am writing more *fully*, in the contents or summary, on points *peculiar* to them, or which I suppose to be

such, than on other subjects, which yet I thought I could not avoid introducing in the Lectures. But I must also have some preface, which shall briefly state what seems to be the relation of quaternions to some other things, especially to double algebra. And it has occurred to me that it might be not amiss, as a preparation, nor uninteresting to yourself, that I should write to you on the chief points of *contrast* between quaternions and double algebra; the *agreements* are obvious on the surface, and it will cost me little or no trouble to draw up a short account of them for others.

'All about differentials, integrals, variations, summations, general theories of surfaces and curves, with physical applications, &c., I must reserve for a future volume. By the time *it* is ready, I may be a little more up to the *history* of the whole class of speculation. Meanwhile you have, on various points, relieved my mind; and don't take any trouble about Gauss at present, so far as I am concerned. Kummer's recent Paper suggests to me, who know *very* little about *numbers*, that Gauss's biquadratic residues may *perhaps* be of the form

$$a + ba + ca^2 + da^3,$$

when $a\ b\ c\ d$ are integers, and a is an imaginary 5th root of unity; but all this is quite out of my way. I may mention, however, that I have heaps of unread Liouvilles, Crelles, and Comptes Rendus, so that a *reference* might now and then be of great use to me. After a little while, I *can* send some more old "baby" verses for Mrs. De Morgan's album.'

From the SAME *to the* SAME.

'OBSERVATORY, *Monday night, January* 26, 1852.

'Is not this an amusing account of the origin of the phrase "Panic"? It is a pleasure, and an useful one, to go off now and then from one's habitual pursuits; and while hunting, just now, among my scattered books and papers, for your essay on triplets, to write to you about, my hand fell on a book in partially contracted Greek, which I opened at this story about Bacchus and Pan. Come, said I, De Morgan knows Greek; this will do as well as anything else. So I set to, as a relaxation, to copy the story

for you; but may well have made some mistake in the *accents*, especially where *un*contracting; and even, though I don't suspect it, in the *sense*, here and there, as I have not consulted any version, lexicon, or grammar, and never met the passage before. I have, *meo periculo*, inserted an *iota subscriptum* or two, which may have been hazardous. Never mind—*duriora passi.* I have found something more interesting, namely, your *first* note to me; but one thing at a time. Good-night.

'(I have not even *looked* at the title-page, but your bibliography may probably anticipate all that).'

"ΕΚ ΤΩΝ ΠΟΛΥΑΙΝΟΥ ΠΕΡΙ ΣΤΡΑΤΗΓΗΜΑΤΩΝ.

"Διονύσου στρατηγὸς ἦν ὁ Πᾶν. Οὗτος πρῶτος τάξιν εὗρεν, ὠνόμασε φάλαγγα, κέρας ἔταξε δεξιὸν καὶ λαιόν· Ταύτῃ τοι ἄρα Κερασφόρον τὸν Πᾶνα δημιουργοῦσιν. Ἀλλὰ δὴ καὶ πρῶτος οὗτος πολεμίοις φόβον ἐνεβαλε σοφίᾳ καὶ τέχνῃ. Ἦν γὰρ Διονύσῳ ἐν κοίλῃ ναπῃ ἤγγειλαν οἱ σκοποὶ μυρίαν χεῖρα πολεμίων ἐπέκεινα στρατοπεδεύειν, ἔδεισεν* ὁ Διόννυσος· οὐ μὴν ὅγε Πᾶν. Ἀλλὰ ἐσήμαινε νύκτωρ Διονυσιακῇ στρατιᾷ ἀλαλάξαι μέγιστον. Οἱ μὲν ἠλάλαξαν· ἀντήχησαν δὲ αἱ πέτραι, καὶ τὸ κοῖλον τῆς νάπης ἦχον πολλῷ μείζονος δυνάμεως τοῖς πολεμίοις ἐνεποίησεν. Οἱ μὲν δὴ φόβῳ πληγέντες ἔφευγον. Τοῦ δε Πανὸς στρατήγημα τιμῶντες τὴν Ἠχὼ τῷ Πανὶ φίλην ᾄδομεν· καὶ τοὺς κενοὺς καὶ νυκτερινοὺς τῶν στρατευμάτων φόβους Πανὶ κλητίζομεν."

From the SAME *to the* SAME.

'OBSERVATORY, *January* 27, 1852,
'*Tuesday night.*

'My dear De Morgan. To save time, may not I as well call you so, and you call me dear Hamilton? I respect your English prejudice against that sort of thing, but your note of 1841, which has just (last night) turned up, reminds me that we WERE once introduced. Of course you will say, we *could* be *only once* "introduced"; but I can prove the contrary, at least by the expressed opinion of a lady to whom I have very long looked *up*, and to whom I used, about a year ago, in perhaps a fit of playful spleen

* ἔδδεισεν would please my eye better.

(reciprocated at the moment by her), some such expression about "introduction." I speak of Lady (Guy) Campbell, daughter of Lord Edward Fitzgerald—a "Pamela" (of Mad. Genlis, not of Richardson)—with whom, to my own great moral advantage, I was intimate an immense time ago, and who always gave me the best advice, and the most friendly counsel—not that she was so much older than myself; and comforted me much about my antenuptial troubles, to which that last line in my sonnet to my garden referred. Well, what has all this to say to you? merely that meeting Lady Campbell at a Dublin Musical party, given (I think) by Mrs. Charles Graves, and at which I met my old pupil the present Earl of Dunraven, who was also a prodigious admirer, in the innocent sense, of the lady already mentioned, I asked Dunraven, in my momentary flurry, to "introduce" me to Lady Campbell, on which she (having overheard me) said, in a pretended huff, "Yes, that is just the word, you are to be *introduced* to me." I must own that in point of fact I had not seen her for two or three years—not since she became a widow.

'Lady Hamilton and I have since visited Lady Campbell, and some of her charming children, who live now more than ten miles away from us.

'Fighting off, for the moment, all thoughts about the quaternions (although some curious thoughts about double algebra passed through my brain this morning), I may add that it was merely as diversion to myself, and as a little mark of sympathy as a University man with you—perhaps you do not know that I am a Doctor of Cambridge ever since 1845—that I copied out, last night, that little Greek anecdote, or myth, for your reading. It would be absurd to boast of *scholarship* on the score of understanding that story. Easier Greek was perhaps never written. I *hoped* that my two boys were gone to bed, at the time of my copying it, but found afterwards that my younger son, Archibald, who is a tremendous bookworm, was down stairs. So I showed him my copy of the Greek, and he translated it off-hand. I begged of him to search in Homer whether there was any case of the spelling ἔδδεισε. He brought me first (this evening) the line θ′, 138, beginning: Δείσε δ' ὅγ' ἐν θυμῷ; but afterwards hit off what I wanted, by producing the line α′, 568:

Ὣς ἔφατ', ἔδδεισεν δὲ βοῶπις πότνια Ἥρη.

I remember writing *duriora*, instead of *graviora passi*. Keep never minding.

'P. S. (1).—The said Archibald and myself read the first six books of Euclid through together, a few years ago, in the *Greek*, *not* skipping the Fifth Book, although I left it to *him* to decide whether we should do so. We agreed in voting that the last proposition of the Fourth Book, about the Quindecagon, was *Analytical*.

'P. S. (2).—Bishop Butler says, in the preface to his Sermons (I quote from Whewell's edition), "Hence an argument may not readily be apprehended, which is different from its being mistaken; and even *caution to avoid being mistaken* may, in some cases, render it *less readily apprehended*." The italics are mine.

'How abominably interlined and *corrected* this note is! Yours is a much better plan, of making no alterations. They don't even add to *clearness* always, although, as you may imagine, it is for *that* purpose, and not for elegance, that I make them. See the quotation from Butler, on the margin of the first page; and pray admire my *modesty* in making it!'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*January* 28, 1852.

'What I return of course I received. You are doing exactly what I want; there is nothing for it but to be full—very full—in the statement, the most dogmatic statement, of all points of meaning.

'The sonnets are very pretty. I shall return them; they seem to be originals, or at least not lately copied (I do not speak of the sonnets, but only of paper, pen, and ink).

'You certainly understand those abominable Greek contractions. I cannot find any reference to this story of Polyænus in Smith's classical dictionary (Art. Pan), though the references are very thick, and several instances of the "panic" *modi* of exciting fear are given. I suspect the story has dropped out of reference books. The story of Pan's horns is curious. It reminds me of the way in which the Hindoo mythologists represent military affairs. In their great war (answering to the Trojan war in Greek, from which all

the great families claim descent) it is said that the enemy attacked in the shape of a serpent with many heads. By comparing this description with others, it would seem that all represent modes of attack, and that the above* represents that the dense column of march formed itself into lighter columns of attack, which is more military, no doubt, but not half so poetical.

'All manner of things prevent me going on to-night.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 31, 1852.

'In an early letter of what I suppose I may call the present "correspondence," you invited me to tell you, in confidence, who it was that I suspected of robbing me. On further reflection, my grounds of suspicion seem weaker than they at first did—perhaps you might be able to remove them altogether; but on the whole, I believe it is best to be silent on the subject, lest the recollection that I had expressed a *doubt* of the fairness of the person I allude to, and with whom I have never yet come personally into contact or collision, might make it somewhat difficult for me to be on cordial terms with him hereafter, if we should ever meet, or engage in any sort of correspondence, private or public. I might have the feeling of having acted unjustly, and be in consequence sore, which at present I am not; nor do I at all imagine myself to be in any danger of having the quaternions attributed to him. Very generally, they have been received by eminent men in these countries, not only with candour, but with generosity—you must allow me to use that word, because I do not see that men who have important and original matter of their own to attend to are bound by justice to examine into the merits of every new speculation proposed, nor, having examined, to praise it, even if it be held to deserve praise. For my part, I should be sorry to think myself *obliged* to read every new work of merit, even among those which I may happen to possess and to be capable of reading. Nor is it my business to tell the world that I admire even what I do admire, unless indeed I am conscious of some specific or general obligation, definite enough to be relevantly told, in connexion with something of my own,

* Referring to an illustration representing a many-headed serpent.

which something, moreover, I am publishing, or have published. Now, Herschel, Cayley, Donkin, Peacock, yourself, and others in England, to say nothing of my Dublin friends, have, as it seems to me, stepped out of their own ways to recognise and encourage my exertions; not that they have not all shown themselves abundantly capable of working in the same line; and you must permit me to consider this as generosity, and as something ornamental in conduct—a flesh-and-blood covering of what must still be the internal framework and support, namely, the principle of justice. In short, I don't set up for being an ill-used man.

'As to my having employed the word to which you objected, in reference to a note of yours, I doubtless had a motive; but till I read over all your letters again with care I may not remember it with sufficient distinctness; nor will it then be worth the troubling you with a statement of it. But as I *do* remember what was in my head, when I "hoped you were satisfied" with the concluding statement about the history of triplets, annexed to my first series of researches on quaternions, in the *Transactions* R. I. A., I may as well mention it briefly. I knew that you had preceded my friends, the Graveses, in your particular conception of triplets, and that I had preceded you in the general conception of sets, and was anxious that I should not seem to be greedy of praise for myself, nor to let my old personal regard for others make me unjust to the rightful claims of you, who were, even as far as letters had gone, at that time a comparative stranger.

'Then again—but this is a delicate point to touch on—I had been made cautious, perhaps sensitive, by my intercourse with poor MacCullagh, who was constantly fancying that people were plundering his stores, which certainly were worth the robbing. This was, no doubt, a sort of premonitory symptom of that insanity which produced his awful end. He could inspire love, and yet it was difficult to live with him; and I am thankful that I escaped, so well as I did, from a quarrel, partly perhaps because I do not *live* in College, nor in Dublin. I fear that all this must seem a little unkind; but you will understand me. I was on excellent *terms* with MacCullagh; was the reporter (of course an admiring one) on his first communications to the Royal Irish Academy; spoke of those early papers of his, in 1832, to the British Association, when it first met at

Oxford; took pains to exhibit the merits of one of his papers on light, in a (subsequently printed) Address from the Chair of the R. I. A., on the occasion of presenting to him a gold medal in 1838, the first during my Presidency, and for the awarding of which to him I had previously spoken and voted in Council, as against a paper of my own (a copy of which Address I can send you, if you have it not); wrote on his melancholy death the sonnet herewith enclosed; followed his coffin on foot from the College through the streets of Dublin; co-operated in procuring a pension for his sisters; and subscribed to the Mac Cullagh Testimonial.

'He was one of the *very first* persons to whom, in the Council-room of our Academy, on the 16th of October, 1843, I showed the then just born equations involving i, j, k. At that time they seemed to him quite new; but about a year afterwards he worked himself into a fever of suspicion, that I had in some way stolen them from a "question" of his own, which was, it seems, proposed by him to the candidates for Fellowship in Dublin, in 1842, and which certainly connected, in a very remarkable way, the *ordinary* $\sqrt{-1}$, with an *ellipse in space*. If I can find the proper Calendar, I shall copy the question below.*

'In this instance, however, as before about the conical refraction, he came to acknowledge my originality, and not merely my independence or priority, but that he had failed to see the things I saw, although it may be supposed that a little farther thought might have enabled him to see them. And it is naturally a pleasant, or at least a comforting reflection, to me, that Dr. Stokes—an eminent Dublin physician, who is also an appreciator of genius, and had poor Mac Cullagh to spend what was the last evening of his life with his (Dr. Stokes's) family and himself, but did not, at that time, apprehend any immediate danger—informed me, as we walked side by side in that funeral procession through Dublin, that Mac Cullagh talked for a long time (he said an hour) about the quaternions, as a remarkable discovery, which he *then* attributed entirely to me. Still, it is possible that *his former* suspicions (arising, I believe, chiefly from the fretfulness of ill-health) may have made *me*, to this day, a little nervous about being *suspected*.

'I find that one of the "Stratagems" collected by Polyænus is

* [*Lectures on Quaternions*, p. (43).]

the well-known order *ora petite* at Pharsalia, ἐς αὐτὰ τὰ πρόσωπα. So I suppose he may be classed with Lucian, and perhaps he *invented* that story about Pan; but I assure you *I* did not invent it, although when a boy I once amused myself by composing (what I could not do now) a Lucianic dialogue, respecting the Rape of Helen, in which I described her tearful irresolution on the Grecian strand, and threw in several pretty incidents, for which I should have found it difficult to assign any other authority than that of the relating sea-nymph. Years afterwards, this dialogue turning up, I had the malice to send it to my poetical friend Aubrey De Vere, without any explanation, and he was delighted to receive the new fragment from the wreck of antiquity, from which it may be judged that his knowledge of Greek prose was not equal to his skill in composing English poetry.

[Here follows an anecdote printed, *supra*, vol. ii., p. 34.]

'The line from Virgil that I lately alluded to was of course:

O passi graviora, dabit Deus his quoque finem.

Query, are the quaternions included among the *his quoque*? I thought I was done with them, but my interest in them has suddenly revived. However, I shall try to stick to the table of contents.'

From A. De Morgan *to* Sir W. R. Hamilton.

'*February* 2, 1852.

'. . . Your account of poor Mac Cullagh's death interests me much; it tends to a notion I have in my head, namely, that your race is somewhat susceptible on such points as that of priority. This is not an *English* reflection upon the Irish—for I am not *English*. I was born in India—so was my father—so was my grandfather—and the three countries—quoad any special association of mine—are undistinguishable—in fact I am a Briton unattached. . . . Dr. Mulcahy of Galway has just sent me a good-looking book about the altitudes of modern geometry. I knew I had heard the name before; and I now remember that in the materials furnished to me for the life I wrote of poor Murphy in

the Supplement of the *Penny Cyclopædia*,* it was noted that the person who found him out was a Mr. Mulcahy of Cork. Is this the same person? If so, I suppose he must be well beyond fifty years old.

'There is another Irishman in whose writings I am interested —I mean Mr. Salmon. What aged man is he? How comes it that the geometrical extensions take such root in Ireland? Yourself, C. Graves, Mac Cullagh, Salmon, &c., are all *up* in them. Hardly a soul in England cares about them. The pure differential calculus does not seem to interest *you* (I mean *you* plural) so much, except as something to be applied.

'Nothing is more common than a suspicion of others copying inferentially—that is, taking ideas from other persons' cognate ideas. Now, in the first place, this is *fair*—it is the way we all must do. A question in an examination paper may suggest—and if it do so, it is right it should.

'Lord Adare I remember—twenty years ago—when he was a boy. He used to be with Dr. Robinson in town, and I used to meet him at Francis Baily's.

'I shall be glad to have the Address you mention. The sonnet about Mac Cullagh is appropriate, and I have no little carping to make on the language.

'When you have quite done with quaternions, you *must* write a book on the undulatory theory—never mind the history of $\sqrt{-1}$. The play is not played out yet. Mine is, however, for to-night.

'P. S.—The remark about caution against mistakes lessening apprehension is wonderfully true.† Have you my "Book of Almanacs"? and my "Arithmetical Books"?

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *February* 9, 1852.

'If you really are as little accustomed to the modern geometry as you would have me believe that you, or at least that the "English" are, you will stare at the following "proof" of a theorem of Desargues,‡ which occurred to me the other day. It

* [A most interesting account of Murphy's early life.—R. P. G.]

† [*Supra*, p. 329., P.S. (2).]

‡ [Friend of Descartes, and author of a treatise on *Conic Sections*.]

brings in $\sqrt{-1}$, but seems to me to have no sort of connexion with double algebra, nor with quaternions.

'The theorem is that cited at the commencement of my Abstract lately sent, namely, that if a transversal cut the four successive sides of a quadrilateral in a conic, and the curve itself, in the six points A, A', B, B', C, C', those points are in involution. By imaginary perspective, substitute for the conic a circle, and for the transversal a line at infinity, perpendicular to the bisector of the angle between two opposite sides of the quadrilateral. Take for unity the perspective from the centre on the line, the foot being made origin of abscissæ, and the line itself the axis. Then the points of intersection with the sides come to be represented by the numbers $+t, +u, -t, -u, \pm t$ being conjugate, as also $\pm u$; and the intersections with the circle, by $\pm\sqrt{-1}$. But whatever i may be, the three pairs $\pm t, \pm u, \pm i$, are in involution; $\therefore$ &c.! Perhaps I have caricatured the mode of "proof," if such it may be called, but I assure you that I should feel no small *surprise*, if a theorem derived in this way should afterwards be shown to be false, by processes of a more ordinary kind.

'Salmon may be called a young man, at least he is *much* younger than I am.

'I think that there *is* a greater, or at least a more general, aptitude for pure geometry in Ireland than in England. The Fellows of T. C. D. are nearly all geometers, and some of them are extremely good ones, although the public examinations for Fellowship do not turn *much* upon geometry—analytics having, I think, in general, a larger share allowed them in the mathematical part of the course; which course is very extensive, and perhaps too miscellaneous. I was very glad to get Mulcahy's book, and immediately set my boys to begin it, which has had the good and designed effect of driving them back to Euclid. Mulcahy, before he was appointed Professor in one of the Queen's Colleges, had long been celebrated in Dublin as a "grinder," or private tutor in mathematics, and was known to be well up in all traditional lore of *cuts*—which I was never taught; for though I always carried away whatever premiums, &c., were going, when I was an undergraduate, I had no private tutor; and, on the other hand, was not obliged by circumstances to become one, although I was afterwards induced to receive two of the young Pagets here, as guests rather

than as pupils, although paid for, during Lord Anglesey's first viceroyalty, and at a later time had Lord Adare as a paying inmate, but rejected all other applications. Lord Adare, now Dunraven, is a distant connexion of mine—fifth cousin by my mother's side—and a very pleasant person he was to have in one's house, besides that he was so fond of astronomy as almost to be a second assistant. I particularly stipulated with his father that I was not to be expected to teach him the classics, but could not resist the impulse to give him some help when I saw him at times hammering away at Greek. One day that some blunder in the grammar (which perhaps I might *now* make myself) provoked me, I asked him, "What *did* you learn at Eton?" "Learn," said he; "I never fancied that I was sent there to learn at all." And as he was a most dutiful son, I believe that he spoke the exact truth.

'I do not admit that the Irish are peculiarly susceptible or rather jealous, at least in matters connected with science, although no doubt they are, myself included, an excitable race. Poor Mac Cullagh was an exceptional case, and his fretfulness arose out of ill-health. I am not conscious of being on terms of even the slightest unkindness with a single Fellow of T. C. D., and with several of them I am on a friendly and indeed affectionate footing. To be sure, many of them *ridiculed* me about the quaternions, and Charles Graves once burst out with the exclamation, in my presence: "It is astonishing what a prejudice exists against the quaternions, and that among people who confess that they know nothing of the subject!" "Would it not be more discouraging," I replied, "if the same prejudice continued among those who *are* acquainted with it?"

'*February* 10, 1852.—Mac Cullagh's question (for Fellowship here, in 1842) was the following, and I think you will admit it was a remarkable one, while yet you will see that the $\sqrt{-1}$ is the ordinary one, and that not even double algebra (at least such as Warren, &c., have used) is introduced, though each equation is obviously designed to include two. After my attention was called to the subject, about the end of 1844, by learning that Mac Cullagh laid a stress upon the result, and on its previous publication, I easily *proved* the theorem—at this moment I forget how—and deduced some others similar to it; but was not able with truth to

say that I had consciously derived from it even the slightest help or suggestion, as regarded the invention of the quaternions in 1843. Whenever *your* great work on $\sqrt{-1}$, from the Hindoos downwards, appears—and *I* have no intention of writing such a work, although I said something playfully about it—this question may properly have a niche in it.

[Hamilton then gives Mac Cullagh's Theorem, and adds, 'I see, since copying the above, a very simple proof of the theorem,' which proof he accordingly appends. Both theorem and proof are to be found at p. (43) of the Preface to the *Lectures on Quaternions*.]

'Salmon is an excellent fellow. Mulcahy must be as old as you conjecture. I have *not* those books of yours which you mention—nor your Algebra; but I can't expect you to give me *all* your books.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*February* 12, 1852.

'I don't see the relevancy of your appendiculum to your answer as to whether you had certain books of mine—namely, that you can't expect me to give you *all* my books. Who said you did or could? I trust you don't mean it as a reflection on my fecundity of printing. I say this because I did collect all I have printed out of serials, and was startled at the mass; and perhaps you may have been startled too at the idea. Nevertheless, I shall send you the books I mentioned as soon as the new postal regulation comes into force about books—on March 1. This sending of books, you know, is done by authors quite as much for their own sakes as for their friends.

'Your imaginary perspective I can admit. Concealed under it is a substitution of the equilateral hyperbola for the circle. An assumption of $x^2 - y^2 = 1$ for $x^2 + y^2 = 1$, is, of course, $y\sqrt{-1}$ for y as a mere instrument of transformation. I have always held it a great pity that the equilateral hyperbola had not some marked property like that of the circle which would have made Euclid introduce them together.

'A year or two ago I wrote a letter to the *Philosophical Magazine*, signed "The General Equation of the Second Degree,"

complaining, in the name of that respectable member of society, of geometers having made it appear as if the ellipse took more than its share of a certain property. Both in the ellipse and hyperbola the locus of the perpendicular on the tangent from the focus is the circle—an ellipse. The ellipse has the lion's share of this property. Now what is the property in which the hyperbola takes both? You will not be long in finding it.

'I shall draw up a small Paper to show how the modern geometry may be worked without ambiguity on the system of *signs* which I gave in the *Cambridge and Dublin Journal*. I have been looking at Salmon's book lately, and regret much that his general propositions are worked on a *case* of the diagram, their generality being secured by trusting in algebra for the other cases.

'As to people ridiculing quaternions, let them do it; but do not let them succeed in making you feel it. They exist and act—as Newton said of gravitation. You take care of your "contents," and never mind the "non-contents."

'If Mac Cullagh imagined that the property of the ellipse he had assigned had anything to do with any system of $\sqrt{-1}$ except the old one—or could suggest anything—he was strangely deceived. The property, so far as appears, has a shade more of connexion with the couplets of the Pure Time than with quaternions. In devising the question, he first established $x^2 + x'^2 =$ constant, &c., and then constructed his equation.

'I object much to the phrase of six points *in involution*.

'If the ratio compounded of AB to BC, and CD to DA give a ratio of equality, we say AC is harmonically divided in B and D. Say, AB, CD are harmonics.

'Then if AB to BC, CD to DE, EF to FA, compounded, also give a ratio of equality, why not say $A\ B\ C\ D\ E\ F$ are harmonics? We have then an harmonic quadruplet and sextuplet, and we might have octuplets, &c.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *February* 13, 1852.

'I was at one time well acquainted with Monge's *Analyse*, if that be the name of the larger work, lately re-edited by Liouville, in which he connects partial differential equations with families of

surfaces, and modes of geometrical generation, and also treats of involutes and evolutes in space; but it is so long since I studied that work, that I laid aside, in a sort of despair, your Paper "On the Connexion of Involute and Evolute in Space," when I received it a few weeks ago. But I took it up for a variety to-day, spent the whole morning in studying it carefully, and think that I now understand it thoroughly. The transformation

$$\xi_{,,}\xi + \eta_{,,}\eta + \zeta_{,,}\zeta$$

in (21) is very pretty; I suppose that in getting it you used the value

$$\sigma'^2 t^{-1} = \xi' A' + \eta' B' + \zeta' C',$$

for I found it inelegant, at least in my hands, to differentiate *one* alone of the three equations (20).

'About the constancy of CPT, the following simple process occurred to me. Merely because PP' and TT' are *small* of the first order with respect to PT (without any use of the *right* angle TPP'), while PTT' is a straight line, the angle $TP'T'$ is small of the second order, and $CP'T$ may be substituted for $CP'T'$. The right angle TPP' next gives (as in your Paper) $TP = TP'$ in length. Thus the fundamental equation of plane trigonometry,

$$CT^2 = CP^2 + PT^2 - 2CP \,.\, PT \,.\, \cos CPT$$

is to be differentiated as if CT and PT were constant, and it then gives

$$CQ \,.\, dCP = CP \,.\, PT \,.\, d\cos CPT,$$

when Q is the foot of the perpendicular from T on CP. Simple as this last formula is, I got it (just now) as the interpretation of an expression for the differential of the scalar of the versor of the quaternion $PT \div CP$. I know that you will not, and ought not, to turn aside at present from your own pursuits to examine any such calculations of mine; but as, *at some future time*, you may be induced to acquire a practical familiarity with their working, I shall here record (if you preserve this letter) a few equations respecting involutes and evolutes which, *after* reading your Paper, it gave me little trouble to form.*

* [These equations are to be found in the *Elements of Quaternions*, pp. xxv., and 621, &c., with an acknowledgment of De Morgan's share in the investigation.]

'I hope that you will be able to look over my "Contents," when a proof-sheet of them is ready from the printers. I am taking pains to mark off distinctly what is *common* to other systems, and, in short, to assist a reader who is not a mere learner to *skip*. As intermediate between the frightful length, but paucity, of the "Lectures," and the shortness, but multiplicity, of the "Articles," I am introducing into the Contents a division into "Sections," § i., &c., of which there are now xxx. written out, representing 153 pages of the book. I am aiming to make the Table of Contents a readable *abridgment* of the work; but must make a few prefatory remarks besides.

'The night before last I attended a grand ball at the Castle, and contrived to pick up a little botany and embryology from Allman, geology from Jukes, and news about a poetical friend from Anster, besides feeling a fair enjoyment of the spectacle, and renewing or forming an acquaintance with two men, officers in the army and navy, whom I had not seen since they were children.

'I met also a niece of Francis Baily, at present Lady Kane, and had a talk with her husband, Sir Robert Kane, about the mathematical professorships in the Queen's Colleges.'

From the SAME *to the* SAME.

'OBSERVATORY, *February* 14, 1852.

'As to Euclid and the equilateral hyperbola, one would think it *almost* as natural to have considered the case of a triangle on a given base, where the *difference*, as where the *sum*, of the angles at that base was a *right* angle. Now Thales is reported (may we believe those stories? I hope we may at least believe that Pythagoras discovered the *property* of the hypotenuse—whatever becomes of the story of the *hecatomb*. Thales, I say, is reported), if I remember rightly what I once read somewhere, to have discovered that the angle in a semicircle is a right angle; and no doubt he knew, if so, the more obvious equality of the sum of the three angles of a triangle to two right angles. The *complementary* relation $A + B = \frac{\pi}{2}$, for the triangle in the *semicircle*, or in the circle, is therefore probably older than Euclid; and it seems that the analogous relation $A - B = \frac{\pi}{2}$ *might* or even *ought* to have very early oc-

curred; but this gives the equilateral hyperbola as locus of the vertex. I suppose the property is *very* well known; it served me several years ago to form a neat equation by quaternions for the equilateral hyperboloid of two sheets—of which perhaps more anon. Another close connexion between circle and equilateral hyperbola is, that in each the diameter and tangent make complementary angles with the ordinate.

'Instead of racking my brains to re-invent your property of the hyperbola (though I did make a guess or two), I hunted among my numbers (unbound, I regret to say) of the *Philosophical Magazine*, for some years past, and at last lit on the amusing and instructive Paper by your friend, "The General Equation of the Second Degree," No. 225 of third series, page 546 . . . which I had barely glanced at when it came out, and did not then know it to be from your hand. It gave me no trouble to see *proofs* of the things stated in No. 1: *real* perspective gives at once from the circle the property of the enlarged ellipse; but the *analogies* were new and striking to me. The results of No. 2 seemed more surprising; I was content to prove them by co-ordinates, with the use of which I was once expert. Not at the first look seeing that you designed the ellipse and hyperbola to have a *common* minor axis, in one received sense of that phrase, I eliminated x', y', between the three equations

$$1 = x'^2 - h^{-1}y'^2 = xx' - h^{-1}yy',$$

$$y'(x-e) = hx'y, \quad \text{and found} \quad [(x-e)^2x'^{-2} =]$$

$$\{x(x-e) - y^2\}^2 = (x-e)^2 - hy^2,$$

which gave $\{(x-e)^2 - y^2\}(x^2 - y^2 - 1) = (1 - e^2 - h)y^2$.

You designed $1 - e^2 = h$, which gives $x^2 - y^2 = 1$.

'(To suppose $x - e = \pm y$ would give $y' = \pm hx'$,

$$x'^2 = \frac{1}{1-h} = e^{-2}, \quad \therefore \text{ (say) } x' = +e^{-1},\ y' = +he^{-1},\ x - y = e;$$

the tangent and the supplemental line seem in this case to coincide, each making 45° with axis of x. I picture to myself an hyperbola, $h > 0$; but of course h may be < 0. I have not considered with any care the meaning of the factor $(x-e)^2 - y^2$. Perhaps you have.)

'I wonder what put the theorem about those supplemental

lines into your head. You have explained what led you to *wish* to discover some such theorem. I am *very* far from knowing all that is (perhaps even quite commonly) known about the conic sections; but it is my *impression* that much still remains to be discovered about them.

'Now for a bit of "dogmatism." It is my will and pleasure to believe, and you need not try to argue me out of it, that a circle is not a degenerate ellipse, but an ellipse a deformed circle. The circle is (I hold) *naturally* simpler than the ellipse—subjectively of course—but no one need seek to persuade me that it is an accident of education our thinking so. It has a "prerogative"—as has the sphere in space. But I am willing to grant that the equilateral hyperbola, and both the equilateral hyperboloids, are also prerogative figures, *very nearly, perhaps quite*, as much so as the circle and sphere, and far above ellipse and ellipsoid. Ditto about the equilateral or right-angled right cone. Accordingly quaternions give charmingly simple expressions for these various *eminent* loci, as *possibly* I may find time and room to print, in what I hope will be my closing sheet, 2 L; although very decently concise equations have long since offered themselves to me for other curves and surfaces of the second order; and for some others, for example, the surface of elasticity, and Fresnel's Wave, when treated by my calculus.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*February* 15, 1852.

'Don't be dogmatical without any need—nobody denies it. The circle and the straight line have a real subjective distinctness, as the exponents of pure translation and rotation, as well as for other reasons. Moreover, though the circle is *an* ellipse, yet it is not an ellipse only. For it would be easy to make a class of curves

$$\int_0^x x^n \phi(x, n).dx + \int_0^y y^n \psi(y,n).dy = \frac{a^2}{2},$$

such that $\phi(x, 1) = 1$, $\psi(y, 1) = 1$, giving a circle when $n = 1$, and *never* any ellipse, but only a most transcendentissimal unintegrable in every other case.

'For like reasons I deny that an ellipse is necessarily a deformed circle, though that is one way of viewing it.

'I believe in projection as a natural and necessary mode of deducing the ellipse. But what distinguishes the straight line and circle more than anything else, and properly separates them for the purpose of elementary geometry? Their self-similarity. Every inch off a straight line coincides with every other inch, and off a circle with every other off the same circle.

'Where then did Euclid fail? In not introducing the third curve which has the same property—the *screw*. The right line, the circle, the screw—the representatives of translation, rotation, and the two combined—ought to have been the instruments of geometry. With a screw, we should never have heard of the impossibility of trisecting an angle, squaring the circle, &c. It is true that the assumption of the screw is very like an assumption of all the point of difficulty; but it is *in naturâ rerum* that some assumption there must be—arithmetic excluded.

'I admit the prerogative of the right cone, right cylinder, and sphere. I doubt about the cone, though, altogether, as not self-similar. The right circular cylinder and the sphere are the right line and circle of solid space; they are also the simple translation and simple rotation of a circle.

'So we have

'Point	simplest translation—right line.¶	
	simplest rotation—circle.§	
'¶Right line . .	simplest translation—plane.	
	simplest rotation—cylinder.	
'§Circle	simplest translation—cylinder.	
	simplest rotation—sphere.	

'All self-similar.

'Now because of the double genesis of the cylinder—a translated circle—we get the screw—self-similar (a very bad word).

'But if you want to bring in the *cone*—which, though rotation of a line, is translation only of *growing* circle, and, moreover, not simplest rotation of line—I do not see why the not-simplest rotation of circle should not come in; and then you get the ring and

the self-cutting ring with the orange and the large pip in the middle.

'I never meant to write so much to night.

'You do not see how I get my *supplementals*. I considered that there must be a property unfound, and looking at perpendiculars

$$y = ax,$$

$$y = -\frac{1}{a}x$$

$\left(-\frac{1}{a}\right.$ in two senses reciprocal of a), I conjectured that the simpler reciprocal formations

$$y = -ax,$$

$$y = \frac{1}{a}x,$$

must play some part. And then I felt about for it—how I don't know; but everybody who has ever played at blindman's buff has some idea of the process, if ever he has had the handkerchief fairly on.

'I have written the inclosed to Mr. Salmon—of whom I know nothing but his books, though that is a good deal, or a deal of good. Read it for the demonstration, and pass it on to him if he is a man to like such things from a perfect stranger. *Entre nous*, since my *grande guerre* with your Scotch synonyme—namesake I mean—I think twice before I open correspondence with anybody unknown to me.'

'P. S. The evolute equations I dare say I shall pick up in the course of the content-reading.

'I have known Lady Kane a long time. When Francis Baily went over to Dublin to the British Association, she introduced herself to him. He hardly then knew he had such a relation. After that she was a favourite of his, and is of many people.

'I have just got Mr. Salmon's book on plane curves; he really is a valuable writer. It is a great comfort to have the journals all sifted and arranged, particularly for persons like me, who never strive after the mathematical newspapers, and whose knowledge is therefore not always entitled to the motto *signatum præsenti notâ*.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *February* 21, 1852.

'The proof for Salmon read, understood, and now forwarded by me to the College; but he may not receive it until Monday. I have said that you seemed to fear it might be thought an intrusion, and left the forwarding of it to my own discretion in this instance; but *I* am sure that Mr. Salmon, who is a very amiable man as well as a good geometer, will take it, as he ought, in good part. I had other things to write about, but must postpone them.'

From the Rev. G. Salmon, f.t.c.d. *to* Sir W. R. Hamilton.

'Trinity College, *February* 24, 1852.

'I have to thank you for your letter enclosing one from Prof. De Morgan. You have judged rightly that I should indeed have been sorry had you adopted the course of suppressing the letter. I answered his letter yesterday, and will now give you, as you wish it, the substance of my remarks on his proof.

'The following demonstration is the same in principle as De Morgan's, but simpler in form:—

[Here follows a proof of the anharmonic theorem.]

'I then took an opportunity of expending on De Morgan (I believe now rather unprovokedly) some pent-up wrath against the English school of geometers generally. They seem, in general, not convinced by the most elegant geometrical proof until it is helped out by an algebraical demonstration. For example, Mr. Walton, of Cambridge, has lately published a collection of problems on conic sections, where he gives himself much credit for replacing the geometrical proofs in mine and other works by algebraical ones. Now it seems to me that there are real difficulties enough in mathematics, without increasing the number by conventional restrictions. Why are we to forbid ourselves the use of any instrument which can help us on? When trying to make out a new theorem I would employ, with equal willingness, algebra or geometry (or quaternions, if only I could use them well enough); and, if any mathematician could invent for me a new method, I

should use it with thankfulness. And if this is the method which I employ in my own researches, I think I should be wrong in teaching my pupils a different system. To employ algebra exclusively would no doubt make the book prettier and more harmonious; but it would train up my young readers in the exclusive employment of a single method when it is desirable to make them dexterous in the use of all, and to set them an example in using all by turns, as most convenient. The case is different when the object is not to teach a beginner how to investigate mathematical truths, but to perfect new methods. Thus I can understand that you would wish to try the powers of the quaternion calculus on all possible theorems; and the less manageable the problem appeared by quaternion methods, the more anxious you would be to make the quaternions overcome the difficulty. But when once it has been satisfactorily ascertained that all manner of problems *can* be solved by quaternions, it seems that a writer on geometry generally ought only to employ quaternions where the proof by them is simpler than by the other methods, geometry where geometry is the simplest, and ordinary algebra where it affords the shortest path to truth.

'I have been led into these remarks, because I think the proof in my Art. 262 is better than that which De Morgan (from love of uniformity and system) proposes to substitute for it. At least you will see (*Higher Plane Curves*, p. 137) how my proof at once gives the corresponding properties for higher curves.

'Can you oblige me with a reference to your proof of the anharmonic theorem? . . .

'Your book will probably make the use of your method more general. At present it is a bow of Ulysses, which no one can bend but the owner.'

From Sir W. R. Hamilton *to* Rev. George Salmon, f.t.c.d.

[from a draft.]

'*February* 27, 1852.

'I think you show your usual good sense in your remarks on the propriety of not being bigoted to any one method in mathematics. For the very reason you mention, I hope to continue applying quaternions, from time to time, to all sorts of geo-

metrical (and also to some physical) problems, but should be very sorry to forget co-ordinates, or not to learn such abridged and powerful notations as you treat of, or to lose the pleasure and the profit of reading occasionally in their own noble language the writings of the Greek geometers. We may live to see a sort of mathematical glossology grow up—a comparative anatomy of the structures of several distinct systems of expression on geometrical subjects. It has amused me to fancy sometimes a demonstration of Apollonius printed in one column of a book; a Cartesian investigation of the same theorem in another column; one on the plan you favour in a third; and a quaternion calculation in a fourth.

'I have been so hard at work in winding up my book, of which I have almost finished writing out an elaborate table of contents, although a *few* articles remain to be added to the text, that I have reserved, not read, your proof of the anharmonic property. But as I understand De Morgan's proof, I hope to understand yours also. Mine, such as it was, appeared in the *Philosophical Magazine* for October, 1846, and in the R. I. A. *Proceedings* for July, 1846.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*February* 23, 1852.

'I received the new page of contents as I was examining the old one. My examination does not lead to any fault-finding—for I recover very easily all I distinctly apprehend—and no table of contents can do more for anybody.

'One point that I wished very much to gain will be gained by the table. Your man who is too old to learn new things, as you know, will, in nine cases out of ten, pronounce that the new thing is useless, if he can get a chance. If the book had been nowhere condensed into a body of distinct enunciations, he would have had a chance, for he would have given his opinion as of a speculation about which nobody could contradict him without reading the book. But he will be a bolder man who would pronounce in that manner, when anyone who looks at the contents can see a systematized body of results. Every book should be provided with some palisade against mere talkers. And among the mere talkers, so far as mathematics are concerned, are to be ranked three out of four of those who apply mathematics to physics, who, wanting a

tool only, are very impatient of everything which is not of direct aid to the actual methods which are in their hands. You will find the table of contents a useful outwork.

'Perhaps you will wonder what I am talking about. But I suspect the Irish to be more given to pure speculation than the English. I have not watched the Astronomical and the Royal Society twenty years without being able to support the preceding thesis. The English savant is a very *practical* animal, as he calls himself. Any kneading of mind for the use of future generations he cannot see the use of. There are a few exceptions in the universities—and but a few.

'The materials of the table of contents will do very well.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *February* 25, 1852.

'It is really a great satisfaction to me to receive, as I have just done, your general approval of my table of contents—so far as you have seen it. Had you objected to the plan a fortnight ago, and suggested any special improvement, I should have honestly set about to act, if I could, on the suggestion; but it is now too late, if I am to co-operate with Charles Graves this year, as I wish much to do. A difficulty about the quantity of small mathematical type required had almost driven me to despair; but just before joining, yesterday, the assembly of doctors and others, Lord Clarendon included, who were to dine in Trinity College (Dublin) at the grand banquet, given by the Provost and Senior Fellows (which really was a splendid affair), I found a minute or two to call at the University Press, and arranged that the contents should be printed off in *half-sheets*.

'Lady Hamilton is starting to attend church in Dublin, and I must seal and sign.

'The dinner was the usual one given at "Commencements," but on an unusual scale.'

From the SAME *to the* SAME.

'OBSERVATORY, *March* 15, 1852.

'The three books have reached me, and are very welcome. Something for you very soon. Meantime let me just ask is that capital word "eliminant" your invention? I have lately been printing . . . "determinants, or as some prefer to call them, elimi-

nants . . ." Of course this cannot be mistaken as claiming any merit for *you*, but ought I to *name* you, or rather might I do so ? '*

From the SAME *to the* SAME.

'OBSERVATORY, *April* 2, 1852.

'I did so flood you with letters and papers for awhile that I should not be surprised if you supposed me to be dead or ill. However, such is not the case. I made, to my own surprise, a speech on Monday last, in honour of the poet Moore, and attended a private concert (with some "dear 500 friends") at the Castle in the evening. It was chiefly sacred music, and was understood to be given for the sake of clergymen and others, who scruple (which I do not) the being present at a ball. To me, who am old enough to remember when Moore's poetry was thought to have somewhat, or indeed a great deal, of a rebellious tone, it was striking, and almost amusing, to hear the final "God save the Queen" immediately preceded by a melody of Moore's, which lamented that the emerald gem of the western world had been (*xyz* centuries ago) set in the crown of a stranger. But I had the honour of being invited, in the summer of 1849, to meet the Queen and Prince, at Lord Clarendon's Viceregal Lodge, in the Phœnix Park, near this place, and a brilliant meeting (for Dublin) it was—combining, as struck me at the time, the attractions of a Musical Soirée, a Conversazione (*sotto voce*), a Court (for there were numerous presentations), and a supper. Well, on that occasion, the chief enjoyment, and the chief part of even the pomp, consisted in the singing and the pianoforte performance of sundry melodies of Moore. So we have her Majesty's permission to admire them; and, seriously, they are not in the least likely to produce any rebellion against her.'

From the SAME *to the* SAME.

'OBSERVATORY, *April* 15, 1852.

'I owe an apology to Mrs. De Morgan, for not thanking her sooner, through you, for her little Paper on Mesmerism in connexion with the treatment of insanity. Will you now present my thanks, and please to add that I do not forget my promise to send some other infantine verses.

* ['The name "eliminant" was introduced, I think, by Professor De Morgan.' Salmon's *Higher Algebra*, fourth edition: Dublin, 1885. *Notes*, p. 342.]

'As you suspect, I *know* very little about mesmerism, but am so far from being a sceptic or scoffer, that I have acted, in some degree, the part of a propagandist, and received some good-natured pity, as such, from Miss Edgeworth, and even from Herschel. Not that I ever witnessed a single experiment in mesmerism. A squarer of the circle said to me, long ago, about some three lines which he had made very neatly to meet in one point, but about whose co-punctuality I was dubious, "Won't you believe your own eyes?"—Not a bit of me. They have been too often suborned, or deceived, by Christmas conjurers for me to place that sort of faith in them. Even in what is more strictly, or more commonly, called *science*, my faith in conical refraction as a *fact* (somewhat impudently denied by Mr. Moore), rests *far* more on Lloyd's *testimony*, than on the experiments of which I was an eye-witness. But I do pretend to some insight into veracity of character, and *cannot* disbelieve *all* that I have *heard* attested about mesmerism. So far as catalepsy and somnambulism, my faith has stretched; but, as yet, the marvels of clairvoyance are beyond its reach; although I have read, with great interest, and a profound disbelief, Dumas' "Mémoires d'un Médecin." Did I ever tell you that Lord Clarendon, one day that the late Marchioness of Lansdowne was sitting between him and me at dinner, and that I happened to be able to supply the *names* of some French novels ("Consuelo" and others), which she had forgotten, turned round upon, and looking through and through me with his piercing eyes, exclaimed, "Ah! Sir William Hamilton, when you get *by yourself* in that Observatory you read something else besides Astronomy!" I am so little of a courtier that I have not yet written my name in their Excellencies' books, since my being a guest of theirs rather more than a fortnight ago. Babbage would have done it next day. Yet I enjoy seeing a Court now and then, were it only to value the more my retirement.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 15, 1852.

'Where are you now in your printing, and when do you come out?

'Get and read, by due snatches, the *History of Physical*

Astronomy, by Robert Grant, just appeared. I assure you, you will find it an extraordinary book for a man quite unknown to bring out. But Robert Grant has looked for both mathematics and history in original sources. I have had a long conversation with the author—he owes his opportunities to having been disabled ten years by a fall. He is a man of business—with no means—and the author of the first complete history of the theory of gravitation is helping his brothers in a parasol business until he can get a clerk's place for himself.

'Looking up points of history about the differential calculus, I came to the following, which is rather amusing:—

'Laplace—who stands out for it that Fermat was the inventor of the differential calculus—explicitly states that he had applied his method to the determination of points of contrary flexure and to transcendental curves. On looking at Fermat's tract on maxima and minima, I see that at the end are two diagrams; 1, a conchoid with a tangent drawn near the point of contrary flexure; 2, a cycloid.

Fermat applies his method to *neither*—nor does he mention the *contrary flexure* of the conchoid. I conjecture that Laplace took the report of someone who looked at the pictures only, and did not read the text which accompanies them, and who took the flexure for granted because the tangent comes near the point of contrary flexure. Is it possible Laplace had done this himself?'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 16, 1852.

'I am growing savage! Southey, with whom I once slid down a part of Skiddaw, and who read to me half a book of his hexameter poem, the "Vision of Judgment"—and very well, I thought, he read it—told me that he considered a proof-sheet to be one of the pleasures of life. I quite agree with him. Judge, then, of my wrath, at finding that the postman has again brought me nothing from my printers! Since Saturday last I have been in this state of starvation. And what makes it more provoking is, that the rascals sent me word that they could afford no Easter holidays; and I have in consequence deprived my little daughter (Helen) and myself of the pleasure of a visit to a cousin of mine at Trim,

Miss Hamilton, who learned on my lap, when an infant, some lessons in Sanscrit and Persian—not to mention Greek, Latin, and Hebrew, or French, German, and Italian, and all things proper for an infant prodigy; the truly prodigious point being that the said cousin has grown up a most sensible, quiet, and prudent girl, or young woman.

'*April* 17, 1852.—This morning came a new slip from my printers, bringing the work near to the end of the 35th sheet (560 pages). About eleven pages, *at the end*, are actually in type, and a woodcut representing the focal hyperbola; but I have found it difficult to resist the temptation of throwing in some *intermediate* articles on other subjects. No doubt this last lecture will be a terribly long one, and may seem too discursive or digressive; but I claim to have had a plan, and while sensible of heaps of faults in style, &c., am approaching fast to the accomplishment of the object that I had proposed to myself.

'Whether I can do more than *lend* you the enclosed paper, containing selections from some of my mathematical examination papers in T. C. D., for the last three years, and designed to exemplify the connexion between Quaternions and Variations, I am not yet certain. But I shall try to procure a few copies for friends.

'As to Sanscrit and Persian, I do not pretend to read them *now;* but my childish acquaintance with various languages may, as I have often since thought, have assisted me in my maturer study of mathematical symbols, and even in my attempts to enlarge the limits of mathematical expression.'

From A. De Morgan *to* Sir W. R. Hamilton.

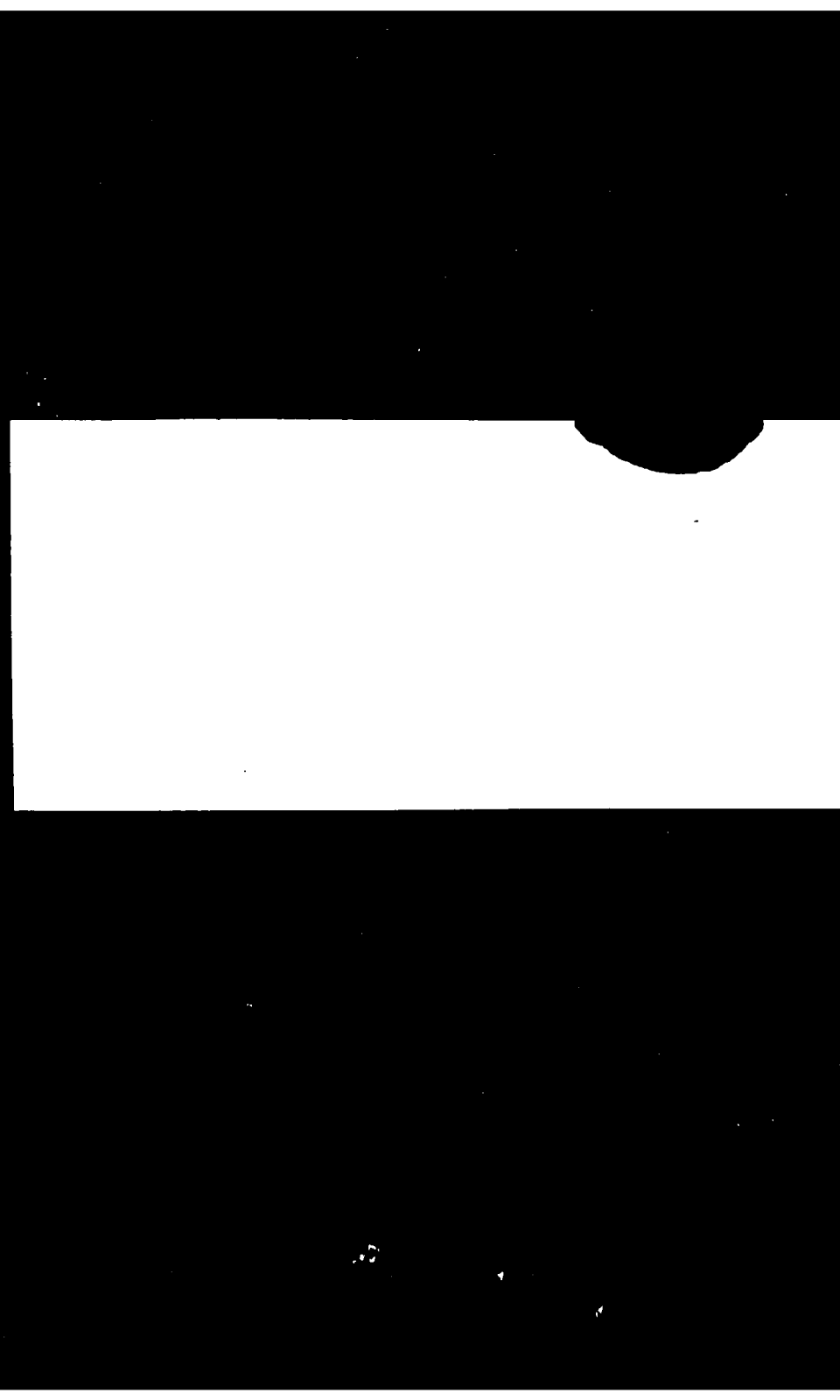
'7, Camden-street, Camden Town,
'*April* 16, 1852.

'Yours received before mine was sent. Some of these days I will send you a simple narration of fact—without any theory attached—on the subject of clairvoyance—and you shall put the theory of probabilities to work to make a string of coincidences of it.

'As to my eyes—or eye—I don't believe them (it) much—never had much reason. But my means of judging of clairvoyance—whatever it is—were direct, immediate, and personal.

For letters which ought to have followed that of April 16, 1852, the reader is requested to turn to p. 623, *infra*.

"Life of Sir William R. Hamilton,"
Vol. III., pp. 352, 353.



'The following good notion of fluxions—and of infinites having other ratio than equality—is from a bishop who died in 1382—(not 1832) :—

'"In every semicircle, the intension of the breadth [ordinate] begins from the utmost degree of velocity, and terminates at the utmost degree of tardity in the middle of the arc; the remission begins from the same middle point with the utmost degree of tardity, and terminates with the highest degree of velocity. But lest anyone should babble about this, I understand *utmost velocity* as in respect of any other which does not appertain to the same kind of figure; for I do not deny that one semicircle begins with a greater velocity than another."—(Nic. Oresmius, Episcop. Lexoviensis) (Lisieux).'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 3, 1852.

'Your questionless answers received, and yourself thanked for same. The roots of an equation determine an equation; but your answers do not determine the questions in some cases. I can, however, make it out well enough.

'It reminds me to return you the proof [of questions proposed at the examination for Bishop Law's Premium]. Your questions are good; but do you examine so exclusively in matters of geometry and its algebra? Your school is too much tending towards curves and surfaces and nothing else; for which you and your pupils will in the next world be turned into long thin snakes, with an intuitive apprehension of the equation of the curve you happen to be in for the time being—and this for ever. You will twist about to try to get your own equation out of your head, were it but for a moment, without any success. You will be separated into branches united by one consciousness, that is, by the feeling that there is but one equation to both; for your personal identity will be the consciousness of having an equation. Think of this while there is yet time for repentance.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *May* 3, 1852.

'You are an *authority* on the subject of *functions;* but I have no need to exhibit *deference* in the present case, for I feel the force of your reasoning. I had not the least hope of my own ever discovering even one value of $(\phi+\psi)^{\frac{1}{2}}$, where ϕ and ψ are arbitrary functional signs; nor indeed do I see any prospect of interpreting usefully $\log.^{\frac{1}{2}}$ or $\sin^{\frac{1}{2}}$ in every one of the many ways which may be imagined to exist. But quaternions, you know, are not entirely *arbitrary* operators. Besides their *distributive* and *associative* properties as multipliers, they have this in common with the old imaginaries, that each is accompanied with a *conjugate*, such that the *sum* and *product* of the two are what I call *scalars*, and are subject as such to all the old and usual rules.

[He then determines the differential of the square root of a quaternion as a case of the solution of a linear equation (the work is to be found in *Lectures on Quaternions*, p. 628 . . . §§ 631 . . . 635), and concludes with a sketch of the subject of §§ 603 . . . 605 in the same volume.]

'At last I make up your Walker, and remain,' &c.

From the SAME *to the* SAME.

'OBSERVATORY, *May* 5, 1852.

'Thank you for remembering my wish that you should return me the little paper of questions. I was not sure of procuring any other copies, but have since been promised some (on paying for them, as I suppose).

'I am glad to have the opportunity of saying a word or two about one of those questions, and that old plague, priority. No one can well imagine, on looking over the printed paper, that I claim anything for myself, except the quaternion analysis employed. Quite lately, as part of the new matter which I cannot resist the temptation of adding to my Lectures, in consequence of the delay of publication, occasioned by what, for the honour of this University, I should be ashamed to plead to the public—a want of proper

type for the Contents—I have written out, with some slight comment, the substance of those questions, to exemplify the "Calculus of Variations in Quaternions." Now, on looking over portions of your *Differential and Integral Calculus* yesterday, partly to verify and profit by your recent reference, I lit on page 443, where not merely the *results* are given, for a shortest line on a surface of revolution, but the same mechanical *verification* (or explanation) assigned which had occurred to myself. I have accordingly pencilled the following memorandum, which I will or will not insert, exactly as you choose, when the proof slips come to hand: "A similar remark is made by Professor De Morgan, in his Differential and Integral Calculus." For my own part, I cannot say that I like to be, on all occasions, "mentioned." I was not pleased at Bronwin's citing, a few years ago, in the *Cambridge and Dublin Journal*, as "Hamilton's Theorem," the very simple formula,

$$f(x)=f(1+\Delta)x^{\circ},$$

whereas I had published, in 1831, this much more general one,

$$\nabla' f\psi(0')=f(1+\Delta)\nabla'(\psi(0'))^{\circ}.$$

If I had not the opportunity of consulting yourself, I should think it more respectful to you to omit, on my own responsibility, the above-mentioned reference to your "Calculus." But I believe that if I *do* print my little "Evolute Investigation," I ought expressly to refer to you the property of the evolutes of curves on a sphere. You are aware, for I think that I mentioned it, that my *general* quaternion formulæ for evolutes in space are not of very recent growth; but the application of *spherical* curves was lately suggested by one of your many papers.

'As to variety of topics, you may have noted that my printed list professed (with truth) to be merely a *selection* from my questions of the last three years. There were several other questions, about definite integrals, and various other parts of analysis, which I have, for the present, suppressed. I am only one of three examiners, for Bishop Law's Mathematical Premium, and do therefore the less injustice if I take occasionally a peculiar line. And you must remember that I am at present a Propagandist. If I could only make *you* a proselyte—to the quaternions of course! Meantime, I am, &c.

'P. S.—As to variety in my *own* reading, I hope to indulge largely in it, when my book is off my hands. I mean, in science chiefly; for, as it is, I do not quite neglect literature. This morning, for example, finding myself a little fatigued with mathematics, I read, in the open air, before breakfast, the whole text of the second volume of the *Curse of Kehama*, and some of the notes. I was not quite pleased to find, at breakfast, that both my boys failed to interpret, what their young sister did for them, on their translating for her the words, this somewhat cramp dictum of Aristotle, disbeliever in Birds of Paradise, which is quoted by Southey (from Henry More) in a note: *ὅτι πτηνὸν μόνον οὐδὲν ἔστιν, ὥσπερ νευσικὸν μόνον ἐστιν ἰχθύς.*

'My copy of *Kehama* has ἰχθὸς (in two places); but I do not remember meeting that form. *Your* books are among those which I hope to attack, as a *reader*, not as a controversialist. Did Henry Crabb Robinson ever tell you that I am slightly acquainted with my Edinburgh namesake? I was introduced to him, at his own wish, in 1830, and took tea one evening at his house. He also pressed on me a sort of indefinite loan of a curious volume of modern Latin poetry, including a long and to us interesting poem by Leibnitz. But I have never exchanged with Sir W. H. even the slightest written correspondence. Had I done so, I should not have felt easy in corresponding so freely with you.'

From A. De Morgan *to* Sir William Hamilton.

'7, Camden-street, Camden Town,
'*May* 6, 1852.

'Your letter received—also Walker, for which many thanks. I see that John Walker could think about logic as well as about arithmetic.

'Neither Walker nor myself has any claim on account of merely incorporating the negation into the predicate. The change implied in passing from "man is—not horse" to "man is not—horse" is as old as Aristotle, or nearly. What I believe I claim is the introduction of two new forms involving contraries, namely—

"Some not X is not Y,"

"No not X is not Y,"

which cannot be transformed into Aristotelian assertions or negations about X and Y, meaning, in truth:

the first, "There is which is neither X nor Y": the second, "There is nothing but is either X or Y."

'The introduction of these and their systematization with the rest I have never found anywhere, before or since I wrote.

'Your dealing with $d.r^{\frac{1}{2}}$ may be quite right. But it is to be remembered that partial interpretations may solve problems. We got on well with the positive square root before the negative one was known. Liouville (I think) solved problems and explained difficulties, with fractional differentials, based on

$$\phi x = \Sigma A_n \epsilon^{a_n x}.$$

So did Peacock in the *Report*, based on

$$\frac{d^m.x^n}{dx^m} = \frac{\Gamma(n+1)}{\Gamma n - m} x^{n-m}.$$

But their two systems did not agree.

'*Thursday.*—Yours received. I would rather not be cited for so little a matter as the revolution surface shortest line. I agree with you, and rather dislike citation on small matters. You can cover yourself by "as has been noted," if you like.

'The theorem

$$fx = f(1+\Delta).x^{\circ},$$

in the old form, is Herschel's: see *Examples of Calculus of Differences*. You stand where the separator of

$$f\overline{x+h} = \epsilon^{h\frac{d}{dx}}.fx,$$

stands with respect to Taylor's theorem. The other, more general one, is yours, I have no doubt.

'Citation leads to queer results sometimes. At the Lady's College in Bedford-square (where my daughter goes) the teacher of mathematics quoted me about logarithms in his class; and my daughter heard one of the girls saying "Oh, I wish Mr. De Morgan had never invented those logarithms."

'Why should you have felt uneasy in corresponding with me if you had done so with Sir W. Hamilton? *I* correspond with him. When he sent me his book (a few days ago) I wrote him a letter ending with the following alternative:—"I hope you will by... prevent my having recourse to the knife, and leave me to cut you

up with the pen as occasion shall serve." And instead of binding me over to keep the peace, he consented to my stipulation. The matter was this: when my "Formal Logic" was published I sent him a copy as his right, he being replied to in it; and he, like a petulant schoolboy, sent me back the book, because he was offended with the dressing I gave him. So, when he sent me his book, in a small part of which (thirty pages) I am mauled, I gave him to understand that I had sent him my book as the right of a person attacked, and that I looked upon the thirty pages as my right, and that if he did not accept two works I sent him (arithmetical books and almanacs), I should feel obliged to cut out my rightful thirty pages, and return the rest. Fill up my blanks above . . . with "accepting my *opuscula*," and you see that it will not read so murderously as might be thought.

'I cannot find ἰχθος in the dictionary at all, and doubt it being a Greek word. I see you trouble yourself to write accents. I find scholars beginning to get rather sick of them, and I hope to see the day when they will disappear, as also the soft breathing'. I did not even know you knew Crabb Robinson. He was at my house yesterday evening—quite hearty.

'As you are doctrinally with the Church of England you can ask a question which, from such a heretic as I am, would be flat blasphemy.

The Greeks, we all know, fought about three ὑποστασεις in one οὐσια.

ὑποστασις	οὐσια.
substantia	essentia.

The Latins have made it three *personae* in one *substantia*. What proof is there that ὑποστασις in Greek ever meant *persona*, or οὐσια *substantia*?

'The place in Hebrews where ὑποστασις is translated *person* is in χαρακτηρ της ὑποστασεως. Now χαρακτηρ never meant *image* as translated: it was a mark, particularly a graver's or sculptor's mark, as in a seal. Why is not this "the *impress* in his *substance*," instead of the "express image of his person"? When you meet with a theologian, you may put him to his defence.

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 8, 1852.

'I shall, in the first place, get off my hands (with your permission) the question you raise, about a part of a verse near the beginning of the Epistle to the Hebrews. On turning to a copy of the New Testament (London, 1850; Taylor, Walter, and Maberly), professing to contain Griesbach's Text, with the various readings of Mill and Scholz, I find, in the first place, that the text of the passage (Heb. 1. 3) is not marked as doubtful. We have therefore, I suppose, the same Greek words in view. Now, I am well aware that χαρακτηρ and αγαλμα are not interchangeable words. (I avail myself of your proposal to omit accents.) But still I cannot think that χαρακτηρ της ὑποστασεως signifies "the *impress* IN his substance"; even if the words were not here followed by the pronoun αὐτου, on the soft breathing of which, as printed, I should lay no stress, if it did not appear to me to be contrasted, by the whole *context* of the passage, with the αὐτου which very shortly follows it. Merely from the Greek, I should infer that the υἱος, the κληρονομος, was *the* IMPRESSION OF the ὑποσασις of the θεος λαλησας τοις πατρασιν. The word χαρακτηρ is used, I believe, occasionally, with a genitive, to denote what we might call the *characteristic* of some thing or person. But when referred, less figuratively, to the notion of a stamp, or brand, or graver's impression, I imagine that χαρακτηρ τινος signifies the stamp *impressed by*, or the one *originally belonging to*, rather than that *received by*, the person or thing of which the name is put in the genitive; or that the τις is the giver, not the receiver, of the impress. The *context* in the verses referred to appears to me *decisive* on this point, HERE, even if my recollection should be wrong, as it easily may be, in what concerns the general usage of the word.'

From the Same *to the* Same.

'Observatory, *May* 8, 1852.

'I wrote to you this morning a note, of this size, as to sheet, on χαρακτηρ, &c. Whether I shall forward it remains to be seen. You would neither like it as an agreement with your views, nor dislike it as a rude opposition to them. But I may think, on

looking over the note, that it had too much the *air* of scholarship; whereas I cannot pretend to much more than a *liking* for the Greek language, and an early and tolerably accurate acquaintance with a comparatively *small* number of Greek writers; increased since as to extent, but doubtless diminished as to accuracy, by subsequent pleasurable but careless hours of reading.

'About the Walker's Logic, I think that after my first exultation, on getting in a small Dublin shop a book which I supposed you to have sought for in vain, had evaporated, I wrote to you to mention, or confess, that I feared it would not interest you so much as I at first hoped; and that I suspected the point about "not-mastiffs" was not *your* point. But you must now be informed, if indeed you are not convinced of it already, that I have hitherto read your "Formal Logic" only as an *entertaining book*. A story goes, that a person, who read more than he digested, once told a friend of his that he had heard people talk of "Euclid," and that he was curious to read the work. The other lent him a copy, and was surprised to find the borrower return it the next day, with many thanks. "What, have you *read* it?" said the lender. "Yes, thank you," replied the borrower. "Read it?—read Euclid* *through* in that short time?" "Oh, if you mean the A's, and B's, and C's, I *skipped all those*." The "Quaternions," as well as the "Formal Logic," may meet with some *readers* of that stamp!

'But, seriously, I do not despair of *yet* reading your book, or rather *that* book of yours, as well as some others. When your "Algebra" reached me, I forthwith attacked the "Introduction," and read it regularly *through*. (I was amused to observe that my son, William Edwin, your acquaintance, despised, or at least *skipped*, the said introduction, as if it could convey no new information, or instruction, or suggestion to *him*, and went on, at once, to the First Chapter; so that, on comparing notes, he was found

* Long ago I was on a visit at the same house, with a most ladylike, but not *very* intellectual Englishwoman, who, happening to hear some of the party talk one evening about the obscurity of some parts of Coleridge's 'Aids to Reflection,' requested to be allowed to take the book to her room. At breakfast, the next morning, she said she had read it through, and could not imagine where the difficulties lay. To be sure it was summer time, and she may have been an early riser!

to be *far ahead* of me! He is no prodigy, but a good and sensible and *reasonably* industrious boy, and may yet do very well.) I also spent a good while, soon after receiving them, in looking over, with much interest, and some amusement, your "Arithmetical Books," and "Book of Almanacs"; and was engaged, for two or three hours to-day, in reading the *early* parts of your "Differential and Integral Calculus," which acquire a new interest to me just now, from my researches on differentials of quaternions. On that subject (differentials) I must write to you again.'

From the SAME *to the* SAME.

'OBSERVATORY, *May* 9, 1852.

'I shall let the note about χαρακτηρ go, but have no wish nor ability to engage in any controversy about it. It was in August, 1850, that I met Henry Crabb Robinson, at Rydal Mount. We were afterwards, for a while, fellow-travellers by railway. He may remember reading to me, out of my copy of Wordsworth's poems, the lines addressed to himself. But it is not unlikely that he may have quite forgotten that we ever met. We had a little talk about Sir William Hamilton, whom I had then lately seen.

'Did you ever see the enclosed Abstract, of date December, 1845? It contained the extended form of Taylor's Theorem, which I had already *applied* at that time. Are you *sure* that the little theorem,

$$fx = f(1+\Delta)x^{\circ},$$

is *not* mine? Just glance at sections vii. and viii. of Herschel's *Examples* again. I am sure that he shall be most *welcome* to the restitution, if the property, such as it is, be his own.

'Is Herschel still at 32, Harley-street?'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 10, 1852.

'As to Herschel's Theorem, which you have translated into the calculus of operations, without knowing what he had done, look at my "Differential Calculus," pp. 307, 308. Your $f(1+\Delta).x^{\circ}$ gives his expansion. In one sense they are *different* theorems.

'I don't remember seeing this particular Abstract of 1845. But it reminded me that years ago you forwarded to me a good bundle of Abstracts to distribute, with instructions, however, not to distribute many till I had some further instructions from you. These have never arrived. And I have only distributed a few.

'As to ὑποστασις—you gave me, with brief interval, two informations, one written, one printed, of your "Church of Englandism," as Bentham calls it. So I tried a pass with you, as you gave me the measure of your sword. But I am no lover of controversy. Remember, I have no doubt whatever that αὐτου refers to θεος. The question was, whether ὑποστασις could be rendered *person*. I maintain it to be a something of his *substance*. Some of these days I will dilate on the mode I intend to employ, when I have time and money, to apply the theory of probabilities to the question whether the anonymous epistle to the Hebrews was written by St. Paul.

'When you tell the story about Euclid, remember that the man left out the "A's, and B's, and C's, and the pictures of scratches and scrawls." I once met a man, no strong mathematician, who said he read Airy's *Gravitation* through on a bench in the front of his house.

'When you read my "Formal Logic," take the Paper in the Cambridge Memoirs—the *second* one *first*. It will give you all the pith—and more.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 12, 1852.

'*I* was the aggressor—there is not a doubt about it—you *could* not avoid paying me back a little. Let me at least plead, on the score of *politeness*, that I did not *intend* firing off *two* barrels at once. When I picked up for you, in December last, the Walker's Logic, first edition, which I had never seen, nor indeed heard of as an *edition*, before, and saw, in print, prefixed, the author's statement of his departure from the Church of England, I felt myself impelled, out of a sort of honesty, to make a memorandum on the page, that I did not agree with him. But the result was, that I came to feel a sort of delicacy about sending to you the book at all, and should probably have tried to let you forget it altogether, but for

your reminding me lately of my promise. My answers to the Royal Commissioners were drawn up, after long delays, in haste, "Raw Haste, half-sister to Delay" (Tennyson), and I suppose that their only non-faulty element is their veracity. Whatever led me to insert the statement of my "Church of Englandism" in *them*, when I sent *you* that unfinal copy, I was merely thinking of the circumstance, that we were freely communicating for the time, and that I had *received* proof sheets from you (of your triplets), as well as *sent* you others. I *request* you therefore to believe that, in point of fact, I did not *wish* to provoke you into an argument, although I grant that I may very naturally have *seemed* to do so. The subject is far too grave for me to treat it lightly; nor do I think that you desire to treat it so. It involves no less than this:—To *whom* are divine honours to be paid? I agree with you (as I suppose), in blaming the ordinary Roman Catholics, especially the poorer ones, for *worshipping* her, whom yet I agree with them in calling the Blessed Virgin. The question, as respects myself, and the Church in which I was baptized, and to which I belong, amounts to this, at least: Are we (the members of that Church of England and Ireland) *idolaters*, in worshipping Christ? in praying, not only *through* but *to* him? *That* is the only question which *I* think *important* in the matter, but to which, *partly*, no doubt, from influences of *education*—although *as little*, I think, from influences of *interest*, as in any other human case—I answer without any doubt; asserting my (confessedly partly *taught*) *belief*, that we *may* pray to Christ; and that, of course, we therefore *ought* to pray to him. You may, if you can, overthrow the received, or (so-called) *orthodox*, interpretation of the word ὑποστασις, and it will concern me very little. You may establish the old doubt of the Pauline origin of the Epistle to the Hebrews. I am not a particularly timid reader, and shall read your publications, on those as on other subjects, if I have access to them. But I am not very likely to adopt your general theological views, and have no hope of your adopting mine. So it only remains that I should repeat my regret for having exposed myself to the charge of having sought to draw you into a controversy which was not likely to be profitable. Who knows but that *we may yet* have some *useful* controversy on some mathematical subject?'

From PROFESSOR DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 16, 1852.

'Imprimis—slip 92:—De Morgan—not de Morgan—when I was at Cambridge, I used to *get* out of my misery in *viva voce* examinations sooner by M—D than I should otherwise have done, by insisting on this capital arrangement.

'Herschel still lives in Harley-street, as before. He is somewhat of an invalid, but I think is better.

'I do not fix on you an attempt to make a controversy. It was I who took advantage of your profession of faith to see whether you had paid attention to *all* the meaning of your profession. But you have subsided into *general* adherence—for a member of the Church of England who does not care for the meaning of ὑποστασις is a great deal nearer to me than is Dr. Pusey, for instance. When I came to think about my M.A. degree, I found I should be required to declare that all who dealt wrongly with substance and person would perish everlastingly; and so I continue B. A.

'If I were to enter upon your modified question of the *worship* of Jesus Christ, I should have to ask whether you mean *worship* in the English of the seventeenth century or of the nineteenth. If in the sense of the man who fell down and worshipped his master, or of the worship of the sciences which Baptista Porta's translator speaks of, or of the *worship* which James I. declared to belong (only) to gentlemen, when he forbad them *honour** (in an edict in which he rates justices of the peace for allowing themselves to be styled *your honour* when their proper title was only *your worship*)—to take the three first instances I remember—THEN Mahomet did, and Socrates would, worship Jesus Christ; and that worship, at least—runs from one end of the New Testament to the other. If you take the modern sense, I should fall back on the practice of the apostles. I am content to go as far as they did. N. B. these alterations of meaning rudely confuse minor distinctions. The *worship* of idols was forbidden, *i. e.* the *minor* mark of respect—*a fortiori* the major.

* I first saw this cited in one of Theodore Hook's novels: he used to rake up such things.

'The inclosed is a puzzle for your daughter.

'We want a name for that collection of n^2 points, real or imaginary, through which pass an infinite number of curves of the nth degree. Is not a *porismatic system* the proper name? Given n^2 points, to pass a curve of the nth degree through them. Generally impossible—in certain cases *one* system—sometimes *two*, but *then* an infinite number. This last is precisely the Greek porism—to the best of our knowledge of it.

'I am now going to turn my attention to writing an article on the case of my friend Libri—a man whose persecution will be the great blot of French science for many a day. The means which have been taken to stifle all discussion of the case in France will give it, I hope, all the more currency in the rest of Europe.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 21, 1852.

'Received, among other things, your scrap* declaring that $\sqrt{-1}$ is of multiple interpretation. I talk so little to mathematicians that I really do not know what they think about controverted points. With the exception of Libri, I have not, since our correspondence on quaternions began, *spoken* so many words to a mathematician on mathematics as I have *written* to you—nor to all mathematicians—they are few and far between—and I have no time to cultivate their society. I should have said, Is it possible anybody can hesitate about this multiplicity? For example, there is an infinite number of interpretations deducible from mere double algebra. Let a certain plane (A) be that of ordinary double algebra; and let another plane (B) be that, say, of transference. Let every point in space determine a curve; that is, let the point (a, b, c) determine the curve

$$x = \phi(v, a, b, c),$$
$$y = \psi(v, a, b, c),$$
$$z = \chi(v, a, b, c),$$

—say the curve of transference (projection). Let a point in A have a corresponding point in B, determined by the curve of transference through A. Then algebra interpreted in A by double

* [This scrap is missing.—R. P. G.]

algebra may be interpreted in B by the transferred or projected lines. Though generally speaking $\sqrt{-1}$ would depend for its meaning upon the point to which it is referred.

The passage from anything to its meaning would involve a $\phi\theta\phi^{-1}$ kind of operation. I remember, when I was writing on functions, I wanted a name for this species of interrupted inversion $\phi\theta\phi^{-1}$, and I asked Whewell for one. He answered instantly, "a *sandwich* of course," which I thought ingenious but inapplicable; though the likening of inverse operations to bits of bread and butter turned contrary ways was a pretty analogy.

'I very much want a short *and sound* demonstration of

$$\int_0^\infty \frac{x^m dx}{1+x^n} = \frac{\cdot\frac{\pi}{n}}{\sin\left(\overline{m+1}\,\frac{\pi}{n}\right)}.$$

The common one is sound enough, but long and too unartistic.

'If for x we write x^p (p positive) we get

$$\int_0^\infty \frac{p x^{\overline{m+1}\cdot p-1}\cdot dx}{1+x^{np}},$$

of which any percentage may be got into $\int_{1-a}^{1+a}$ by making p large enough; so that ($p=\infty$) it is an instantaneous integral, and a may be infinitely small. But I do not see any simplification in this—and here I leave it.

'I saw this morning, for the first time, the original publication of Fermat's method of maxima and minima, tangents, &c., in a volume of Herigone's Course of Mathematics (1644). It is another instance of the manner in which a person's first ideas will smell of his predecessors more than his later ones. His increment (e) is an absolute 0. Let $e=0$, substitute $a+e$ for a in ϕa, &c. This was Kepler's view. "The infinitely small increment," says he, "absolutely vanishes at the maximum or minimum."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Royal Irish Academy, *May* 24, 1852.

'I have just been told that I am likely to be called on to give some account this evening of a subject begun by me some time ago,

Continued Fractions in connexion with Quaternions and *Bi*quaternions. The title of this subject was not printed as part of the bill of fare for this evening's meeting, and I came in to Dublin not at all expecting to come on. It will be well if I can *remember* the leading formulæ, or at least the leading points, in time, especially as I am going, in half an hour, to dine with the Academy Club, which I do not often do. So I shall merely add at present, 1st, that when I said I was sure you were right about multiplicity of interpretation, I was thinking rather of

$$\left(\frac{d}{dx}\right)^{\frac{m}{n}}$$

than of $\sqrt{-1}$; for I agree with you that the interpretations of *it* are probably infinite; 2nd, that I have read your recent proof of

$$\frac{\pi n}{\sin \pi n} = \int_0^\infty \frac{dx}{1+x^n},$$

and think it quite satisfactory.'

From the SAME *to the* SAME.

'OBSERVATORY, *May* 26, 1851.

'Do you know that I am not *sure* that I may not *meet* you next week at Greenwich? For some years past the compliment has been paid of inviting me to be present at the Visitation, and to dine (paying, of course) with the Visitors afterwards. The annual card arrived by post this morning, and I am tickling my fancy with the notion that *this* time I may *act* upon it. *If so*, I suppose, we shall be for an hour or two together (on June the 5th). But if so, I must *return*, perhaps that very evening, without any opportunity of paying my respects to any of my acquaintances in London. It must be, as concerns myself, what the "Herschel Dinner" at the Freemasons' Tavern was in 1838, when I just went to London for that one purpose of attending it, and returned, the purpose being accomplished. 'Tis true that I was provincial enough to visit St. Paul's, and to climb into the Ball—where the strange effect of the wind made me think that it was "the sighing of the Heart of London." Something to that effect I wrote that evening, on board a steampacket, to the late Marquis of Northampton, with whom I was on what might fairly be called intimate

terms: with the present Marquis I am merely on terms of a distant civility. (He was not at Castle Ashby when Lady Hamilton and I made our visit to his father there, in the autumn of 1838). But about the "sighing"—am I to quarrel with Dickens, or Dickens with me, because he printed almost the same image or figure in one of his publications of a later date? Where is this priority business to end? I am as sick of it as you can be; but still, in anything important as regards science, I should take it as a favour to be *warned*, if I were inadvertently exposing myself to the charge of plagiarising. As to verse-writing, I remember copying for Lady Rosse, in 1848, a sonnet, such as it was, which I had composed by starlight in the highest gallery of the great telescope at Parsonstown; and noting that one line resembled some verse of a living poet, to which I was, just then, unable to refer. The only tolerable part of my sonnet was the conclusion, namely,

> "Pursuing still its old Homeric march,
> Northward, beneath the pole slow wheeled the Bear;
> Rose overhead the vast Galactic arch;
> Eastward the Pleiads, with their tangled hair;
> Gleamed, to the West, far seen, the lake below,
> And through the trees was heard the river's flow."

'Now, as to the "tangled hair" of the Pleiads, I cannot swear that this may not have been, in some dim and half-conscious way, suggested by the "Locksley Hall" of Alfred Tennyson, at the time forgotten by me. He has, if I *now* remember—for I won't stir to look for the book—something like this:

> "Glittered like a swarm of fireflies tangled in a silver braid."

'Shall I dash boldly, before I stop, into some new verses of my own? They are certainly not worth offering to Mrs. De Morgan, for whom, without prejudice to her title to receive, whenever I can recover them, some more of my old *baby* lines, I am about to copy a couple of sonnets of my own; and to whom I request you to present them. *These* are for your *own* amusement, and are now to be written from a remembrance of what I extemporised, between the acts of shaving yesterday, for the entertainment of a brother rhymester, . . .

'The verses, then, that I thus *dashed* off, on the spur of the

first jingle between the sounds of *splashing* and *mashing*, were an account, for the said brother versifier, of a dinner of the Royal Irish Academy Club, on Monday last (the Queen's birthday) while a flag was flying, in honour of that day (May 24), from the top of Nelson's Pillar; and they were these—if I can remember them rightly :—

'I helped the soup without any splashing,
Also the salmon without much mashing;
Cut up the fowls in morsels nice,
And served about the melting ice;
And, seeking every taste to please,
Scooped out the ripe and rotten cheese.
Then, rising from the Chairman's seat—
But first our Dean had blessed the meat,
And in our names due thanks had given
For all the boons of gracious heaven—
I called on all the social Board
Their hearts and voices to afford,
With loyal and with glad acclaim
To welcome a majestic name;
And, on our sovereign's natal day,
In cordial, earnest, toast to say:
Long be her years, bright and serene!
Victoria's health! God bless the Queen'!

'That the decanters did not circulate *too* fast, nor too often, may perhaps be inferred, in my case, from the circumstance that I was called on, that evening, with scarcely any notice, and spoke for about an hour respecting the *Bi*quaternions, receiving all sorts of compliments afterwards from the President (Dr. Robinson) and the Academy. Seriously, they open a new world of difficulty, for some future Alexander to conquer. It is honour enough for me to have indicated the direction.

'I must tell you that my daughter behaved very well yesterday —though, just before the operation, she whispered to me that she *should* like to be put asleep. She had been given her choice. You would have mesmerized her; I was content with chloroform, and the dose given was not enough to prevent her from having a sort of dreamlike consciousness. She is doing very well. My cousin Hutton is a skilful surgeon.

'I *walked* out from Dublin, after the evening meeting—more than five miles. The night was fine.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*May* 27, 1852.

'I shall not be at the Greenwich dinner, which I shall much regret, if you are to be there. My lectures are so late that I cannot undertake to be there. And in truth there I have never been, nor ever was I at a meeting of the Royal Society, nor at the British Association. In fact, I am the most ungregarious animal living.

'My wife will be much obliged to you for the verses, which are very pretty, and I say ditto for the dinner rhymes. But you cannot keep to Dean Swift's vein, you walk out of it into loyalty, &c., and it was a mercy you did not amble into sonnet metre, and give one the idea of an inversion of "desinat in piscem mulier formosa superne"; setting out with Swift's head and shoulders, and ending with the feet of one of the Muses.

'I am very glad your daughter is out of it [a slight operation near the eye], I hope for good and all. Where chloroform can be ventured upon, it is more speedy than mesmerism, which requires some previous trials. There is a kind of consciousness left by both, very often. The first man operated upon (amputation of the leg) by mesmerism in England said "he felt a *crunching*"—it was the *saw!*—"but it did not hurt."'

From the Same *to the* Same.

'7, Camden-street, Camden Town,
'*May* 31, 1852.

'You get your answer by return of post—for this is Whit Monday, and here I sit—holiday-making, literally puzzling myself—or rather having puzzled myself—with the following question, for two minutes, by mere forgetfulness of geometrical extension:—

'How is the end of the shadow of this pole* to be determined?

'On looking into several books of perspective, I see that they take care not to mention the case—always having the sun high enough to avoid it.

'How does your daughter get on?'

* [The shadow of a high pole cast by the setting sun.]

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *June* 2, 1852.

'. . . Herschel wrote me a very kind letter lately, saying that he was *so* glad to see the "Contents," as a token of approaching *publication*, and reminding me of the shortness of life. I *do* remember *that;* but so possible is it to apply a motive in two ways, that the remembrance makes me all the more anxious to bring out *now* what I *hope* (you will smile) to publish this very month, as full an account as other claims on my time will allow of the state to which the quaternion calculus has been already brought.

'This morning I rose about six; took some tea standing, for I was too busy to sit down; deciphered some half-effaced characters, which I had chalked upon a blackboard, as lecturing myself, a few months ago, on the subject of a new sort of *variations of definite integrals* (in quaternions, you understand), *not* depending on any *infiniteness* of the *function* between the *given limits of integration*, and therefore not anticipated, so far as I yet see, by Cauchy. *Vide* your Calculus, or his Memoir, perhaps a rare one, on definite integrals between imaginary limits (Paris, August, 1825), which, if you have not got it, I shall with pleasure lend you, besides returning your Mourey—but wish to have both a little longer. In that Memoir, at page 57, Cauchy refers to a formula of "M. Guillaume Libri, dans le tome 28 des Mémoires de l'Académie des Sciences de Turin." Is *he* your illustrious and unfortunate friend? Something, but very little, too little, I know of *an* eminent Libri's researches, for instance, respecting linear differential equations; but have none of them at hand to refer to.

'To descend to myself, I went after the lately mentioned deciphering, and after getting something copied for me, on my car to Dublin, with my eldest boy, procured cap, gown, and band at the place where such things are kept for me near the College; arrived at the large and handsome hall, or theatre, devoted to examinations, elections, &c., and in which the new Provost and other members of the Senior Board were assembled, for publicly conducting the usual Fellowship Examination; proceeded to the seat in the inner circus, which Brinkley used to occupy, and to which I had successfully vindicated my claim, by the single process of staying away, when, about twenty years or a few more ago,

Provost [Bartholomew] Lloyd (an excellent man) excluded me from it, for a year or two, on the grounds that I was too young, and too much at home in this my native University, not to sit with the Junior Fellows; heard the statutes and oaths respecting the election read aloud in Latin by Dr. Todd, and was amused by his involuntarily falling back occasionally into that language afterwards, when, novelty of novelties! he proceeded to examine *in English* on what is here called "Logics," including Aristotle, Kant, Hobbes, Priestley—pray admire the chronological order!—Locke, Stewart, Brown, Reid, Lord Monboddo, and Sir William Hamilton. (There were others; but I missed De Morgan.) How strange it sounded to hear all this discussed in the vernacular!

'Would M. Libri accept a copy of my book when published?'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*June* 8 *and* 9, 1852.

'In your remarks upon the result of substituting $x+y\sqrt{-1}$ for x in $\phi x=0$, and the n^2-n supernumeraries, you give a meaning to $\phi(x,y)=0$, as the equation of a curve, in *double algebra*. This I conceive to be bringing the uncircumcised into the inner court of the temple, for which Paul got nearly stoned, I think; but we live in more quiet times.

'When x and y are *real*, they have no interpretation, except *upon* the unit line.

'A quantity of common algebra is only *quantity*. One of double algebra is both length and position. Every determinate symbol, when it is measured from a determinate point, symbolizes a determinate point.

'Let us now mean by small letters—lines on the unit line; and let large letters be quite general.

'What then is this circle?* Answer, the locus of

$$x+\sqrt{a^2-x^2}\,.\,\sqrt{-1} \qquad \text{[}a^2>x^2\text{]}.$$

According to conventions, every point here signified is on the circle, every point not here signified is not.

* [Here is inserted diagram of a circle with radius a, central rectangular axes, and an ordinate corresponding to an abscissa x.]

'If you like, *on the unit line*, to take $y = \sqrt{a^2 - x^2}$, and then to give $\sqrt{a^2 - x^2}$ its quarter-turn, you may.

But $$y = \sqrt{a^2 - x^2} \qquad [x^2 < a^2]$$

is only interpretable in the unit line.

'When $x^2 > a^2$, then

$$x + \sqrt{a^2 - x^2} \,.\, \sqrt{-1}, \text{ or } x + \sqrt{x^2 - a^2}$$

is all in the unit line.

'Accordingly,

$$x + \sqrt{a^2 - x^2} \,.\, \sqrt{-1}$$

is—1. $[x^2 < a^2]$. The circle—any point in the circle

2. $[x^2 > a^2]$. Any point in the unit line—got in one particular way.

'*Example.*—It is required to divide $2x$ (in the unit line) into two *algebraical* parts, whose *algebraical* product shall be a^2 (in the unit line).

'*Answer.*—$x + \sqrt{x^2 - a^2}$, and $x - \sqrt{x^2 - a^2}$.

'Here it is plain' [from consideration of the figures] 'that a problem has its locus in the circle and the unit line conjointly:

circle for $x < a$,
unit line for $x > a$.

But now we ask what does

$$X + \sqrt{A^2 - X^2} \,.\, \sqrt{-1}$$

represent? *Answer*, if X be perfectly unlimited, any point whatsoever, but only as obtained in one particular way, or set of ways.

'Under what *restrictions* is its locus the circle? The question here is, given a curve and its representation, required the co-ordinate system under which the given curve shall have the given representation.

'Any expression may belong to any curve, if the proper co-ordination be adopted.

'In common algebra, length only is represented, and position understood; consequently, only permanent positions can be kept in thought as co-ordinate.

'But any two lines which start from the origin, and end at a given point, may be co-ordinates of that point.

'If $X = x\varepsilon^{\xi\sqrt{-1}}$, $Y = y\varepsilon^{\eta\sqrt{-1}}$ be the co-ordinates of a point, the system of co-ordination is defined by two equations:

$$\phi(x, y, \xi, \eta) = 0,$$
$$\psi(x, y, \xi, \eta) = 0.$$

These equations are, in the common system,

$$\xi = 0, \qquad \eta = \frac{\pi}{2},$$

which we bear in mind, but not as expressed conditions. So in plane geometry, my pupils bear in mind, but not expressly, the *second equation* of all the curves, $z = 0$, that is, they always keep in the plane; but when we come to solid geometry, they always find it difficult to remember that their curve in xy is now *expressly*

$$\begin{cases} \phi(x, y) = 0 \\ z = 0. \end{cases}$$

Let r, θ be the common polar co-ordinates of a point. Then

$$x\varepsilon^{\xi\sqrt{-1}} + y\varepsilon^{\eta\sqrt{-1}} = r\varepsilon^{\theta\sqrt{-1}}$$

or

$$x \cos \xi + y \cos \eta = r \cos \theta = x_1,$$
$$x \sin \xi + y \sin \eta = r \sin \theta = y_1.$$

If $y_1 = \chi x_1$ be the common equation of a curve, we have then *three* equations to the curve, as we ought to have:

$$\phi(x, y, \xi, \eta) = 0, \tag{1}$$
$$\psi(x, y, \xi, \eta) = 0, \tag{2}$$
$$x \sin \xi + y \sin \eta = \chi\,(x \cos \xi + y \cos \eta). \tag{3}$$

The common three equations of a straight line are

$$\begin{cases} y\varepsilon^{\eta\sqrt{-1}} = a \,.\, x\varepsilon^{\xi\sqrt{-1}} + b, \\ \eta = \dfrac{\pi}{2},\ \xi = 0. \end{cases}$$

Hence, given one of the four, length or direction of one co-ordinate, the others are found.

'If χ be given and ϕ and ψ unknown, it is a functional

equation with two unknown functions to make a given equation

$$\omega(x, y, \xi, \eta) = 0,$$

combined with (1) and (2), give (3), which is the problem of making a given equation represent a given curve.

'But what do we get when we generalize x and ξ, &c.? Only a covert way of choosing another system.

'If $x = x' + x''\sqrt{-1}$, &c., our co-ordinates are

$$(x' + x''\sqrt{-1})\varepsilon^{\xi' + \xi''\sqrt{-1}} = x\varepsilon^{\xi\sqrt{-1}}$$
$$(y' + y''\sqrt{-1})\varepsilon^{\eta' + \eta''\sqrt{-1}} = y\varepsilon^{\eta\sqrt{-1}}$$

and $\phi = 0 \quad \psi = 0$ become four equations. But we ought to have six. If we take six equations at pleasure, between $x' x''$, &c., we have a seventh in

$$x \sin \xi + y \sin \eta = \chi(x \cos \xi + y \cos \eta),$$

where χ, &c., are functions of the eight. But this can be reduced to the first considered case—for the six equations of co-ordination, together with the four which express χ, &c., enable us to eliminate $x' x''$, &c., and leave us two equations between $x\ y\ \xi\ \eta$.

'What then, you may ask, do you mean, in double algebra, by the equation $\phi(x, y) = 0$ in its most general sense? I reply that, first, you have no right to make *real* values of y perpendicular to those of x, unless you expressly mix two systems of double algebra, as D. F. Gregory and Walton have done in *Cambridge Mathematical Journal*, and as I shall do to a greater extent in the next number. These duplicate double algebras do good service in interpretation, but do not mix in one *algebra*.

'To interpret, for instance—

$$x^2 + y^2 = 1,$$

or rather

$$X^2 + Y^2 = 1.$$

First, X and Y being real, each solution symbolises a pair of points in the unit line.

'Next, x being $x' + x''\sqrt{-1}$, and y being $y' + y''\sqrt{-1}$, each such solution symbolises all the points derivable from

$$x'^2 - x''^2 + y'^2 - y''^2 = 1,$$
$$x'\,x'' + y'\,y'' = 0.$$

To make this give a curve, we must have recourse to definite co-ordination, as above described.

'If we want something more like the theory of curves, we must make some hypothesis such as Gregory has made, *e.g.*, Let y be measured in a plane perpendicular to that of x, its real part being the $\sqrt{-1}$ part of x. Then, for the point Q,

$$X = OM + MN \cdot \sqrt{-1},$$
$$Y = NP + PQ \cdot \sqrt{-1}.$$

'The equation $\phi(X, Y) = 0$, then belongs to a surface, and $\psi(X, Y) = 0$, to another. *All* the intersection of the two surfaces does not belong to the system $\begin{cases} \phi = 0 \\ \psi = 0 \end{cases}$, but only those points of it which are symbolised by the same co-ordinates. Thus the point Q may be on both surfaces, but may not be the property of both equations for the same co-ordinates—being on one for

$$OM + MN\sqrt{-1} \text{ and } NP + PQ \cdot \sqrt{-1},$$

on the other for

$$OM + MN'\sqrt{-1} \text{ and } N'P + PQ \cdot \sqrt{-1}.$$

'I foresee that some of these days partial solutions of equations will be considered. For example, given $\phi(X, Y) = 0$, $\psi(X, Y) = 0$, required the solutions of the two equations which agree in the real parts of both, or in the imaginary parts, or in the real parts of one and the imaginary parts of the other.

'This gives complete interpretation and objective existence to the mn points of the intersection of two curves of the mth and n^{th} order—which is all that is wanted for full investigation of the various cases.

'Your aspiration after some new world to conquer in double algebra I cannot join in. I have no objection to any extensions; but when every operation is actually explained, I have what I want. You *want* a *geometrical instrument*—well, you have made one; but algebra—arithmetic in the widest sense—is not made for geometry. There is a law of thought to consider; if, upon a preconception of what we wish it to do, we make it an instrument for a special purpose, we divert attention from our main object, the consideration of that law of thought. I hold that double algebra must remain as the full development of the conceptions of arithmetic, so far as those symbols are concerned which arithmetic immediately suggests. The invention of other symbols is legitimate.

'The great feature of the complete algebra is geometry introduced for explanation of algebra—not algebra adapted to geometry—and the great use of the extended field of explanation is that in algebra—the science of quantity by symbols—we *reason* throughout. I never hope to see the day when there will not be a science of *symbols* in agitation, in which extended power of *interpretation* is the thing we are in quest of, but the leaves of the ledger which are made up are not, I hold, to be reopened, though new accounts may be opened.

'Are you sure that if you were to go on with your *bicouplets* you would not land in quaternions, which I have always hoped would turn out to be the legitimate triple algebra, or algebra of space.

'I have a clearness, as the quakers say, that double algebra is in and of a *surface*, not necessarily plane; that single algebra is the true *linear*—double the true *areal*—and that a real triple algebra is to come. I shall rather quarrel with nature if your quaternions with the *three rotations* be not the triple algebra. This is the point I want to investigate.

'I did once try a kind of bicouplets. Seeing that a and b when understood go into $a+b\sqrt{-1}$, where $\sqrt{-1}$ is a new kind of symbol, I thought the next step would be to imagine

$$a+b\sqrt{-1}+(a'+b'\sqrt{-1})\,.\,k,$$

where k is some new kind of quantity. But I made nothing of it. All the laws of algebra were exhausted, and there really was nothing for k to do that I could give it.

'Now you have tit for tat (what does this phrase mean?) as to quantity. And I intend to set to upon some old notions about discontinuity and the principle of mean values.

'To-day it is making up for the drought. It has rained incessantly for eighteen hours and more.

'Are you much of a politician at elections? I never gave a vote in my life.'

From Sir W. R. Hamilton *to* Professor De Morgan.

'Observatory, *June* 12, 1852.

'Thanks for your printed paper, and long note, both of which reached me this morning. Ten days (or nights) ago, I wrote to

you a double-sheeted note, but laid it aside with the purpose of adding to it, and now—not that I pity you on that account—there seems but a poor prospect of its ever turning up again. For you must know that I gave a sort of official dinner in my Library on Wednesday last, as being then (for the 26th time) one of the Examiners for Bishop Law's Mathematical Premium, and was obliged to allow housemaids to use pretty freely their discretion in clearing out the room. Orders were given, no doubt, to abstain from destroying papers; but to all practical purposes, many, indeed most, of those which were lying about are hopelessly hidden from my view; unless, indeed, I shall be roused, after my book is out, to "take stock," as tradesmen call it, in various ways, as regards volumes, manuscripts, observations, &c. The last are not my property; but I fancy that of my papers a vast number must be not worth preserving. Still whatever *are* preserved may with advantage be classed.

'*June* 14.—I write in the printing office, and have not your last note beside me, which I regret, for there was something in it that I wished to answer, as indeed there have on several occasions been, when I could not write till it seemed too late. Let me at least mention that I have just signed 2 R, in which are just introduced, with brief explanations and proofs, some results respecting that sort of variation of a definite integral, arising from non-commutativeness of multiplication, in virtue of which (as I think I mentioned some months ago) we have, in quaternions, such formulæ as

$$\delta \int_{q_0}^{q_1} fq\, dq = \int (\delta fq\, dq - dfq\, \delta),$$

or more fully,

$$= \int_0^1 \{\delta f q_t \,.\, q_t' - (fq_t)' \,.\, \delta q_t\}\, dt,$$

where

$$\delta q_0 = 0, \quad \delta q_1 = 0.*$$

That subject I have finished, for the present, and have written out some investigations respecting differentials of functions of quaternions, more general than that respecting the differential of a square root.

[An example is then given, to be found in *Lectures on Quaternions*, § 634, p. 630].

* See *Lectures on Quaternions*, § 627 (p. 624).

'I shall be happy to be allowed to present you with two or three copies (or perhaps more) of the book, when completed, for *undistinguished* scientific friends of yours, or pupils not yet known to fame. As to scientific men of *celebrity*, I need not say that I shall take it as a favour, and indeed a service, if you will assist me in making out (soon) a list of such as presentees. *How far* I can go in that way I am not yet certain; but am aiming to be liberal.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*June* 15, 1852.

'The matter you refer me to in 2 R (slip) is new, I have no doubt. I don't see who is to get on the ground, except yourself.

'The parties I remember in Great Britain who are likely to read your slightly abstruse book are, in England—Herschel, Peacock, Babbage, Whewell, Leslie Ellis (Camb.), Stokes (Camb.), Graves, Sylvester, Cayley, Spottiswoode (Oxf.), Kirkman, Young (Havil, Great Southampton-street, Camberwell), O'Brien (King's College, London), Wheatstone (who is curious about such things). I wish you would send one to Libri. I will answer for the charges against him being malignant in their character; and, so far as definite, fully answered by him. I hope to send you a few notes on his case shortly, in print. He is at Florence House, 3, Chepstow Villas, Bayswater, London.

'In Scotland, Kelland (Edinb.), Wm. Thomson (Glasgow).

'I dare say there are more who will come into my head. But I do not live among the mathematicians, and new ones start up. So no more this evening.'

From the Same *to the* Same.

'7, Camden-street, Camden Town,
'*June* 20, 1852.

'All the people who can read the quaternions are sure to be of name; and therefore it is difficult to propose a person who would not come into your own *à priori* list. Do you know Dr. Logan of Prior Park? He is a person who could and would read it. I forget whether I named him before. He has the history of mathematics for 1700–1852 better than any one I know.

'Libri is the writer of the Paper you mention, and a great many

others. Besides being a first-rate mathematician, he is *par excellence* the historian of mathematics who knows the manuscript æra. His history of mathematics, &c., in Italy, of which four volumes are published, is a work which will live.

'His is a lot with a good many alleviations. All the bibliographical world throughout Europe is not only convinced of his innocence, but of the extreme malice and ignorance of the accusation.

'Imagine John Smith accused of stealing a certain book, printed at London, as appears by the title-page; in which book, on the fly-leaf, is pasted the book-plate of John Jones of Manchester. Imagine our Attorney-General—English or Irish—insisting on it that John Smith pasted in the book-plate of Jones of Manchester to persuade the world that the book was *printed* at Manchester, and not in London. This is imagination; the following is reality:—

'In Libri's possession was found a book printed at Venice, as appears by the end—to which everyone looks in an old book for place and date. At the beginning, in the usual place, was stamped in the name of the convent of St. John of the Canals at Piacenza, to which the book once belonged. The *experts* (or professional bibliographers) employed to examine Libri's books declare that *he* stamped in this *book plate* to make it appear that the book was printed at Piacenza. They then point out that Venice is *at the end*, and wind up with "De tels faits ne se discutent pas—il s'exposent!"

'It is a considerable alleviation of such a charge that the knaves who conduct it are such fools. Another is that a lady to whom he was engaged—and a very charming woman—did not forsake him, but came over to England and married him. Another is, that this lady has about £1500 a-year; and her daughter, by a former marriage, has or will have as much more. Another is, that his own property is in Tuscany, where the French cannot get at it.

'You probably know Mr. Kirkman, who extended, or rather augmented, Pascal's Theorem. He has just published a book of mathematical mnemonics, the most curious crochet I ever saw.

'Some of these days I will try to write a catechism of mathematics in which every answer shall be remembered by a double meaning, *e. g.*:

'*Q.* What is a rational state of things?

'*A.* One in which all radicals are exterminated.

'Of course you will not forget the following libraries:—The scientific societies, the colleges, and Sion College, London, which you may never have heard of—a clerical library—the librarian is a good mathematician; Advocates' Library, Edinburgh; Observatory Library, Greenwich: a fair one and in high order. Airy is the prince of *methodists*. You and I should look very small before him. My theory is that when he tries his pen on blotting-paper he makes a duplicate by the pressing machine, files, and indexes it. When he wanted communications of advice and suggestion about the altitude and azimuth instrument, I sent him an edict from Jupiter, warning him not to follow his (Jupiter's) daughter about. It was read to the visitors, and filed with the rest.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *Wednesday Morning*,
'*June* 23, 1852.

'I intend to make *many* presents, including foreign ones; you are aware that I wish to give a few copies, through you, to promising pupils or other private friends of yours, who are not known to the public as writers. It is not my expectation that for the otherwise published copies there will be an extensive, much less a rapid sale; but with all its faults, and with all my belief that, if it were now to be done over again, I could write a much better book, I still think that it will always be regarded as a sort of classical work, in one department of mathematical science, which shoots forth already ramifications to *almost every other* department, and from which I have also strong expectations, not likely to be realized by *myself*, as regards electricity, magnetism, and all that depends on *polarity*.

'Your story about Airy is capital.

'Mr. Kirkman has sent me his book—it *is* a curiosity—perhaps it may be useful to new men; but I suppose I am past using it. If he can make me *remember* the formula,

$$\cot a \sin c = \cos c \cos B + \sin B \cot A,$$

which I am forced to *copy* now, for I have vainly tried, for (to speak moderately) thirty years, to commit it to memory, he will have achieved a feat. Yet I can repeat long strings of lines from Milton, Wordsworth, and others, not to mention all, or nearly all,

the sonnets that I ever composed—and you are far from knowing their number.

'You asked me lately whether I took any interest in politics. I am not an *ardent* politician, and most unaffectedly distrust my own political sagacity, and warn my boys that I do so. It is likely that you and I would be found to differ, without bitterness, on those, as on theological subjects. But since you give me the opportunity, I shall add that, on the principle attributed to Solon, I have always voted at every *contested* election, in the College or city of Dublin, since I had a right to do so; and hope to assist soon in getting rid of Reynolds. (Who was it that said, "I have no prejudice, but I hate a Frenchman?") I have just received a circular, requesting me to vote for the present members (Hamilton and Taylor) for the *county* of Dublin, which I shall certainly do, if I find that I have the right. Hitherto I have always imagined that my tenure of this Observatory did not give me a vote in the county. And now perhaps I have said enough about politics. I may just add that I would not have made any *exertion* to displace the last ministry; but in *voting* I feel as if I were on a jury.'

From A. De Morgan *to* Sir W. R. Hamilton.

'*July* 1, 1852.

'I want to lodge the following somewhere by way of *publication*, a word you understand I use in the primitive sense. There is no mistake which introduces so much confusion into scientific history as confounding *publication* with *printing*. It is only a little phase of algebra applied to logic. . . .

[Here follow fourteen pages occupied with a treatment of syllogism considered as a composition of relations, &c. It concludes characteristically thus:]

'And so much for genus and species. Now for a play upon words. No doubt genus always possesses species *ex vi terminorum*. To prove that it is not lawful to take a letter out of a word, and insert it in another, proceed as follows. *If possible*, let the second *s* in the second word be changed into *i* and inserted in the first. Then, genius always possesses specie. But this is absurd, therefore, &c.—Q. E. D.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*July* 11, 1852.

'The heat here has been so oppressive that men, women, and children are dried up into mummies. If you have anything heretical in mathematics, or orthodox in theology, to insist upon, now is your time; I can oppose nothing.

'Chasles has published one volume of his *Géométrie Supérieure*. I have not had courage to look into it yet. Do you know anything of—(1) Liouville's edition of Monge; (2) Poinsot's Theory of Rotation (1851)?

'There is, of course, no hope of news from a mummy—as I claim to be, and to remain till we have a thunderstorm, or something to alter the air. I was amusing myself the other day with a work on logic, or rather against logic, written by one Justin Brenan. I suppose he must be an Irishman. Do you know anything of him?'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *July* 12, 1852.

'Though I don't much like copying, even from myself, I think it may entertain you to see a copy of part of a note which I have just written to that cousin of mine, who learned, when she was a child, and when I was able to teach such things, some Hebrew, Persian, and Sanskrit, on my lap. Perhaps also it may be less unpolite to you to send the gossip in this form, than if I were to address it directly to yourself, who probably take a different side in politics.

'"My dear Bessy, after thinking and writing about the *bi*quaternions on Saturday morning, and giving William Edwin [*your* London acquaintance . . . parenthesis addressed to A. De M.] a long lecture on practical astronomy, I shaved—an exertion! and went to Dublin with him on my car, from which we got off, and walked as we approached to Green-street. A strong force of police preserved quite enough of order. I am not sure that I should not have enjoyed a little more of a *row* [pronounce like *now*], and I easily found the proper tallyroom, and a person to

show me the way to the proper polling booth. The following questions may not be reported *verbatim;* the answers are nearly so :—

'"*Q.* Under what title, or by what number in the Registry of Freemen do you claim to vote at this election? *A.* 1399. *Q.* (After inspection of the Registry.) Are you the person registered under that number? *A.* Yes. *Q.* Have you already voted at this election? *A.* No. *Q.* For whom do you vote? *A.* Grogan and Vance. (*A voice*)—Thank you, sir!

'"And so ended my little part in the important drama of the day. I walked about Dublin a great deal with Will [your said acquaintance], and part of the time with Sydney [a sister of mine], and thought that my native city looked really very lovely. The Liffey was pretty full, which makes all the difference in the world between *its* beauty or deformity; and the sun was shining on it, and on many beautiful buildings. Even the reflection from house-windows, towards evening, had a fine effect. (Some windows were smashed, by-the-way, as I was slowly driving, about half-past five, on an open car through a crowded street; but that was to be expected.) I availed myself of my first free day, since the Moore Meeting, to leave a card at Charlemont House."

'The remainder of my note to my cousin relates to money and other private matters. I have heaps of things to write to *you* about, and in fact have at least two rejected or suspended notes to you, thrown aside, though perhaps not a bit more foolish than this one, and I *may* yet take courage to send them. Will you believe that I have only just discovered that the Bi-tensor of a product of Bi-quaternions is = ± the product of the Bi-tensors?'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*July* 14, 1852.

'Whether I am for or against Grogan and Vance I can't tell: 1. Because I have not the least idea of their politics; 2. Because I have not the highest degree of certainty of my own. I have no objection to give Lord Derby a chance. I should be glad to think that he or anybody else could get through *business.* I should like to reduce the House of Commons to its original function of granting supplies and stating grievances, leaving the executive to remedy them on *principles* to be settled by the House. I foresee

that the present machine will break down under its own incompetency. I am glad you have kicked out the obstructive man.

'I admire the innocence of your mode of information—that *row* rhymes to *now*. Let the circumstance be fairly stated. An *Irishman* writing from *Dublin* of an *election*, admits that he would have been glad of a *row;* and thinks it necessary to inform his correspondent how the fourth emphatic is pronounced. Not exactly so, perhaps, for I understand that the parenthesis about pronunciation was intended for me. You did not, I presume, think it necessary to add that little bit of instruction in Irish to the Hebrew, &c., which you formerly gave your fair cousin.

'There has been no contest in my borough, and I did not vote when there was one. I hate the system. Given two persons of whom I know nothing; required which is the best qualified to manage matters of which I know next to nothing. The presumption is that 5000 incompetent persons, by a contest of opposite incompetencies, will produce a competent decision. This absurdity fills the House of Commons. Another, as great, the House of Lords, namely, that the son of a legislator must be fit to be a legislator. And this works well, on the whole. I have long been of opinion that, in English hands (not objecting to a moderate mixture of Irish and Scotch), anything would work well; and that Parliament as now constituted proves it. I would not have all Irish, because the progress of events—which should be in a *row* —meaning one after another, with order and method, rhyming to *slow*—might be subjected to a mispronunciation. Nor would I have all Scotch, for, when they had their own matters to manage, there is no denying that they mispronounced quite as much as the Irish. It was only, in all time, when they had to work their way among foreigners, that the Scotch were the decent quiet people which we know them to be. Are you of Scotch origin?—by name and arms you seem to be so.

'Macaulay used to maintain that the House of Commons must have been the beast of number 666. The 658 members, by his showing, + the number of officials requisite to the action of the House—I forget who they were—made exactly 666—just taking in the clerks, sergeant-at-arms, &c., essential to its records, the enforcement of its orders, &c.

'And so much for my politics.

'I shall send you, in a day or two, a separate copy of an article in *Bentley's Miscellany* on the Libri case. There never was such a mess as these unfortunate French have got into.

'There is another light of Italian mathematical history just beginning to appear in England. I received yesterday, from Prince Boncompagni, the person in question, three tracts on points of Italian history. One of them is on Gerard of Cremona, the first translator of Ptolemy from Arabic. It contains (among other things) an Arabic algebra, translated by G. of C., in which there is notice of negative quantity, a thing hitherto wanting in all Arabic algebra. This is a step towards the connexion of Arabic and Hindu algebra. The latter abounds in negatives.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*July* 16, 17, 1852.

'I have found the value of ∞. You are required to admit three things:—1. That $89° 60' = 90°$; 2. That $\tan 90° = \infty$; 3. That any man, having a point left incomplete, may make it up by ancient authority.

'If you boggle at the third postulate, I refer you to your namesake at Edinburgh, and his followers, who never hesitate at imposing a law of thought as necessary, if Aristotle has declared it. I am not joking. If you or I can see no necessity they refer you to Aristotle to prove it.

'Supposing you now satisfied on the third head, I refer you to Reinhold's *Tabulæ Directionum*, 1554, in which is the first *canon fœcundus*, or table of tangents, that ever was carried to single minutes.

'The last degree is to every 10 seconds. Now I quote

$$89° 59' 50'' \qquad 206185567010$$
$$89° 60' \qquad 10000000000000;$$
$$\therefore \qquad \infty = 10^{13}.$$

Everything has its reason. Here is a kind of companion to the *totus sinus*. Since 10000000 was the greatest sine, and it was found constantly requisite to divide 10000 . . . by a sine; and since the same 1000 . . . has often to be divided by a tangent, Reinhold seems to have thought that a *total tangent* would be a desir-

able thing; and so he put down 10000 to as many ciphers as the row would take.

'I had written most of the preceding when your note came in, wherein June 23 appears as date. If you had sent it at once I would at once have given you *my* mnemonic for the formula you mention. The clue is

$$\cos \div \sin = \cot.$$

Now choose your beginning, say

$$a \quad c.$$

Make a cycle of pairs

$$a \quad c \qquad c \quad b \qquad b \quad a.$$

Change the last three into angles

$$a \quad c \qquad c \quad B \qquad B \quad A.$$

Now,

$$\cos \div \sin = \cot.$$

Put cosines in the middle, sines on the flanks, and cotans on the extreme flanks,

$$\begin{array}{c} a \quad c \quad c \quad b \quad b \quad a \\ \cot a \sin c = \cos c \cos B + \sin B \cot A \\ \cot \quad \sin \quad \cos \quad \cos \quad \sin \quad \cot. \end{array}$$

This is the only formula for which I have ever been obliged to invent a mnemonic. Mr. Kirkman says his is better than mine. I have not looked at it—you may. All I know is that I never forgot the above *rule* from the time when I first made it. And in mnemonics the difficulty is always *custodire custodem.*

'Could you not make a sonnet out of the formula? There are just 14 elements,

$$\begin{array}{c} \cot a \sin b = \cos b \cos C + \sin C \cot A \\ 1 \ 2 \ 3 \ 4 \ 5 \ 6 \ 7 \ 8 \ 9 \ 10 \ 11 \ 12 \ 13 \ 14. \end{array}$$

'This evening we have rain and thunder. And—as Sir Sydney Smith (the Acre man) insisted on it to a friend of mine—the large rain-drops knock down the wind, and make it blow in the lower regions of the atmosphere.

'Pray do not write, and then consider whether you shall send it or not. If I were to do such a thing you would not get all you get out of me.

'*July* 17.—I have been hard at work all day valuing the affairs of an assurance company, and incidentally committing high

treason—imagining the death of the heir to the Crown; inasmuch as the Prince of Wales has had his life assured by someone, and, the rate of interest being 3 per cent, if you take the premiums at anything less than thirty-three and one-third years' purchase, you imagine his death.

'But I opened this letter not to expose you to the hazard of misprision of treason, but to tell you that I send a copy of the Libri article with it. You will now see how the land lays. There are a great many more facts, but I had not room for them. And all that I have stated I have as good reason to know can be proved as it is possible I could have.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 20, 1852.

'I received this morning your letter of July 16, 17, and also your pamphlet about Libri, which I have read with great satisfaction, but intend to read again. And I do not think that I thanked you for the one of July 14, which *promised* me that pamphlet, and contained some words about politics. On this last head I do not think that we should differ much—certainly not so as to prevent, or render difficult, a good-humoured correspondence, now and then, upon the subject (whatever might be the case with Theology).

'About a year ago I happened to meet, in Mrs. Charles Graves's drawingroom, a Dublin lady of some talent and a *blue*ish tinge, who set herself to draw out my opinions on various points. Among other questions, or half questions, she said, "You are a Liberal in politics, I believe." "Really," said I in return, "I do not know exactly what the word means; but as it sounds as intended for a compliment, I suppose that I had better accept it." Yet I regularly voted against O'Connell, and *that* not only when Lord Normanby (at whose hands I received knighthood) was Viceroy, but also when his co-candidate, against whom I voted also, was Robert Hutton, now of Putney Park, near London, whom probably, or at least perhaps, you know, and who is my maternal cousin in the second degree. I must say that Robert Hutton, on the occasion I have in my mind, came up to me as I was waiting for my turn to vote, and, knowing well what I intended to do, shook hands with me in public. About a week afterwards I received an invitation, in Dublin considered a "command," to

dine with Lord Normanby, and sat next Drummond—you know perfectly whom I mean. We had met before, at the measurement of the base, beside Lough Foyle, in 1828, and had enjoyed a Sunday's stroll along the banks of that lake, in addition to dining together under a tent, and some other little incidents, extremely savoury to me at the time, of a semi-military life. In our after-dinner chat at the Viceregal table, at that later period of which I spoke before, we did not shrink from politics, although in some important points we differed about them, and the subject of the ballot was mentioned. Whether Drummond was *for* the ballot, I forget, but remember that I was led to say, *against* it, "With what satisfaction or comfort could I, as a gentleman, sit now at Lord Normanby's table, if I had voted *secretly*, instead of openly, against his candidates in Dublin a week ago?"

'My "liberalism," if an existent quantity, won't prevent me from voting, if alive and well, for Hamilton and Taylor, at the Dublin County Election, on Thursday morning next. I belong to the new sect of Derbyites; but I trust that you believe that I would have voted exactly alike if Lord John Russell were still in power, namely, for just those candidates for whom I have voted, or intend to vote, at present.'

From the SAME *to the* SAME.

'OBSERVATORY, *July* 20, 1852.

'Letters and pamphlet received—more very soon from me. Guess, if you can, what *you* have to say to the enclosed receipt for Sir *J.* Hamilton? I am the person meant, and the basket, with *eight* compartments—a practical biquaternion—is for holding letters from, and some few memoranda of letters *to*, you! Already it has been, with great resulting pleasure, applied to the said purpose; but a tradesman waits in my hall, and, while paying him his bill, I shall bargain for his posting this memorandum.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*July* 23, 1852.

'You meant me to wonder at the receipt, and I did wonder, till your note explained it. I congratulate you on your methodical habits.

'I saw to-day, with great satisfaction, an article in the *Times* on Libri's case, of which article I knew nothing whatever. The article I sent you will be circulated by Libri himself, and copies will go to Ireland to a great many official people. I begin to hope that people in this country will be brought to see that there is no criminal justice in France. But there is no one to collect such jewels of criminal law as the following:—

'1. Five men robbed a diligence, many years ago; three men were convicted and guillotined; some years afterwards three more: now $3 + 3 = 5 + 1$, therefore one innocent man was guillotined. Which it was was discovered and fully proved. In fact, the second trial exonerated one of the first three. The family of that unhappy man cannot get his sentence reversed to this day. The Courts would not do it. The late House of Assembly would not recommend it. It was against the dignity of justice, they said, to reverse under such circumstances. All persons admitted that if he had been at the galleys the sentence would have been reversed, and he would have been set free; but as the mischief was wholly irreparable, the dignity of justice required that the stain on his friends should continue.

'2. A man is tried for secret society work. Evidence, a policeman, who swears that certain respectable persons in the man's neighbourhood told him that the man was a Socialist. Prisoner's counsel require him to name these parties. The Court overrules the question. The man is convicted.

'Nine men out of ten whom I meet with have more or less of idea that a refusal to go and stand a trial in France is an inevitable presumption of guilt. One thing, however, I have learnt from all the remarks I have heard on this case, and that is, that the law of England is far above the average Englishman in its notion of justice, and that a study of the *law* would improve the *logic* of nine out of ten.

'I do not see your name among the office-bearers at Belfast. Do you go? Belfast reminds me that the treatment of Young was more French than English or Irish either, except under religious bigotry. I hope the Queen's Colleges will be free of this plague.

'By the way, is it held true in Ireland that the priests educated at Maynooth are more National and less Papist (in the literal

sense of the words) than those not so educated? I have heard this affirmed, and made a reason for the continuance of the grant, and not by Roman Catholics.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 23, 1852.

'You won't think it necessary, I suppose, to enter a formal protest against what I said (if I remember it), when I last (or lately) wrote on the subject of politics; nor to disclaim the *degree* of agreement between you and me on that head which I assumed *potentially* to exist. You need not take pains to inform me that you are not a *Derbyite*, in the sense in which I avow myself to be one: it is quite enough for me, that you state yourself to be "willing to give the Derby Ministry a fair trial," or that you have used words to that effect. We agree in preferring (*immeasurably*, as I hope and think) England to France, and to Italy, as regards constitution and government—witness the "Libri case." You could not, any more than myself, contemplate coolly, as an endurable thing, the conquest or the possession of the British Empire by Louis Napoleon, or by the Pope. You might probably prefer, theoretically, the absence of all alliance between Church and State; may even wish and hope to live to see any such alliance dissolved, but would not choose to throw public affairs into confusion, and paralyse, or at least obstruct, all vital actions of the national body corporate, by any such course of conduct as may, to judge by the past, be adopted in the next Parliament by the Brigade. You may be, in some very important respects, a Radical; but, if so, your intellect, good sense, and good feeling, must oblige you to be (if I may coin the term) a *liberal radical;* while I on my part claim to be (what I have been called) a *liberal conservative.*

'My liberalism did, however (as I warned you), not prevent me, or rather, as by me interpreted, it urged me on, to attend at Kilmainham yesterday, and to vote there for Hamilton and Taylor, who, I am told, are certain to come in; but after an arduous contest—for the Romish priests are understood to be actually canvassing—a mild word for *their* mode of getting voters to the poll! At all events, *liberavi animam meam;* and of course I should

have voted for another Hamilton (G. A.), and Napier, in the College, if there had been any contest.

'I write this note standing in the open air, and have just now (since writing the above) been told that Hamilton and Taylor are IN, by at least 500! Tar-barrels, &c. in this neighbourhood!! No more common sense this evening!!!

'James Hans Hamilton I am long acquainted with, also with George Alexander Hamilton; and though not so long, yet well, with Napier. But Grogan, Vance, and Taylor, I am not aware that I have ever *seen;* and certainly I am not such a *red-hot* Protestant as Vance professes himself to be. But I act on the practical principle of taking the best I can get. If I had not voted for Vance, I should have helped to bring in Reynolds.'

From the SAME *to the* SAME.

'OBSERVATORY, *July* 26, 1852.

'Understand, first, that I don't pretend to be an unprejudiced man. Deeply prejudiced I know myself to be; not thereby admitting that I am wrong. From childhood I have had political leanings, and always to the *il*liberal side. My father (Archibald Hamilton, of Dominick-street, Dublin) was a liberal, almost a rebel; he assisted Hamilton Rowan to escape from prison, and deeply involved himself by other efforts in his favour. On the other hand, his brother, my uncle, the Rev. James Hamilton, who lived for forty years the Curate of Trim, and died as such, was a Tory to the back-bone, and doubtless taught me Toryism along with Church of Englandism, Hebrew, and Sanskrit—that last acquisition being pretty well lost by this time, although I still like the *look* of the letters! My father used to enjoy the provoking me into some political or other argument, in which I always took my uncle's side. The seal, which I still use, and which contains a sort of abridgment of my father's arms, was given me by a maiden aunt, Miss Hutton, on the occasion of my triumphantly winning, as my father more triumphantly confessed to some friends (or flatterers) whom he had collected, in a contest of eloquence with himself for my *first watch*, which, when I was about twelve years old, he affected to deny that he had promised me. Perhaps, before I finish this note, I may be able to lay my hand upon my father's seal, and to send you an impression of it. I should think

it pompous in me to *use* one so large and grand. We *do* claim to be Scotch, of the time of James I.; but believe that our pedigree can be traced back beyond the Norman conquest, to certain Earls of France. What nonsense! I try to think so, practically, and am quite sure that I am *glad* that I am the son of a younger brother, and that *if* a dormant baronetcy in our branch of the Hamiltons shall ever be revived, it will be for a cousin, the *present* Rev. James Hamilton of Trim, son of my uncle above mentioned, and not for me. A poor knight is bad enough, but a poor baronet is something lower than ridiculous.

'What set me off into all this rigmarole! Oh, I remember now, you asked my opinion, as an Irishman, about the Maynooth priests, and I wanted to disclaim any pretence to being an unprejudiced man.

'An amateur genealogist asked me, about a year ago, what branch of the Hamiltons I belonged to. I answered that I had heard my father say, that he had claimed, and been allowed to claim, a distant cousinship with the Marquis of Abercorn; but that I had never heard my father profess to be in any way connected with the *Duke* of Hamilton. "That is all as it should be," said the genealogist, "for the Duke of Hamilton is not a *Hamilton* at all."'

From the SAME *to the* SAME.

'OBSERVATORY, *Monday Evening, July* 26, 1852.

'I should have no chance as a candidate for the *City* of Dublin (whatever might be the case with the *College*, if I were only such a fool as to allow myself to be proposed), because I could not *promise* to vote against Maynooth. On the whole, I am disposed to let the endowment remain. Such is the result of a very unskilful attempt of mine to strike a balance between opposite evils. I am old enough to remember foreign-bred ecclesiastics of the Roman faith. They were extremely agreeable people, and much welcomed in Protestant society. The case is very different now. The education at Maynooth is, I believe, *anti*-English. But I repeat that I do not set up for being an unprejudiced man.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*July* 27, 1852.

'I see you begin *gemino ab ovo*—i. e. from Louis Napoleon and the Pope. Concedo. I do not want England to be governed by either. So far we go together. I am not sure I would destroy them. You have read "Pickwick." You remember Mr. Weller, sen., reasoning on final causes, says of death—"What would tho undertakers do without it?" Now I say—"What would the newspapers do without" L. N. and the P.?

'Practically I hold as follows:—People seem to be well satisfied with the Constitution, as they call it. *I* don't believe in the Constitution. I think this is the country of common sense, and we should make anything do. But, be the Constitution what it may, it is not my business to tinker it; for if it were I should not know how, except in a few small matters as follows:—

'1. The Parliament not to be dissolved all at once, but $\frac{658}{7} = 94$ members to go out by rotation each year, and so that every man should serve seven years. The Queen to dissolve out and out, over and above, *pro re natâ*. The rotational electors to be distributed through the year.

'2. No person to be allowed to go to the poll who shall not have given notice of his intention twelve calendar months before the election. In the event of no such notice given, the House of Lords to nominate $2x$ candidates for x to be chosen, out of whom the choice to be made.

'3. Every member of the House of Commons to swear, on taking his seat, that he has neither aided nor abetted bribery; and to give in, on oath, a list of his election expenses—all to be paid* before he takes his seat.

'4. Votes to be taken at the electors' houses, by papers left to be filled up and signed (blank filling being permitted). These things would not suit the magnates who fool the mob septennially, but out of that *raison de plus*.

* The House to shorten, at discretion, the term of service of anyone who spent undue sums, even honestly.

As to Church and State; if the deadly poison of *subscriptions* be abolished, I should care little how they arranged their matters *inter se.* These subscriptions foster every kind of dishonesty.

'As to the Maynooth Grant, if the Catholics settled down again into good subjects, I should continue it. And this because the idea of it being unlawful to countenance idolatry, &c., contains a principle subversive of all things established. If the state conscience—a creation of politicians—must not encourage what it thinks false, how can the real private conscience be forced to do so? A Quaker has a much better right to refuse Church rates than the State has to refuse an allowance of public money, otherwise desirable, on the ground of its disapproval of the doctrines taught in consequence. No more this batch.

'I picked up the other day the tables of Reinhold, containing (1554) the first table of Tangents (*tabula fœcunda*), published to minutes. He remarks that this table will be found very useful when one sine is to be divided by another, the two angles together making a right angle.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 28, 1852.

'I was much amused by your mathematico-political letter, which came to hand this morning. How many people do you hope to convince, or win over, to your new Utopia? Though it is only fair to admit that you, like myself, seem to be rather glad than sorry, that neither of us is obliged to try his hand, in practice, at "tinkering the Constitution." If a peer, or a peer's nominee, before the Reform Bill, had thrown out your suggestion No. 2, about the nomination of the $2x$ candidates, what a popular clamour would have been raised! The "curry" business (Norfolk, was it?) would have been nothing to it. As to No. 3, against bribery, I am really delighted to think (what I have heard) that a recent Act of Parliament has almost anticipated your view, and made that crime nearly impossible. Yet I do not relish the thought of multiplying *oaths.* For my own part, I have on several occasions been called upon to take an oath, and trust that I have always done so solemnly and veraciously. But when, for instance, those questions about the Dublin city election had recently been put to

me, and I was again walking with my son William Edwin in the streets, I really had to *reflect*, for a short time, whether I had answered on my oath or not. The duty of veracity, at least when so publicly and solemnly appealed to, had appeared to me already so stringent, that no ceremony could increase it. The State, in its assumed capacity of conservator of public morals, has, I believe, assigned a special punishment to perjury. Most rightly, fitly (I admit), is *that* crime punished by any well-constituted society; yet *as* a *crime*, not as a *sin*, by *them;* although an awful sin it *be*. So I would amend your motion, No. 3, by substituting some such enacting words as these, "that a false statement on the subject of election expenses should be treated and punished *as if* it were perjury." But I repeat that I am told that a reform, with which I can *thoroughly* sympathise, has actually been of late *effected*, on this point of bribery; and that the next House of Commons will, *so far*, be one of unprecedented purity.

'At least I can say that it would surprise as well as pain me if I were to learn that a single person had been bribed, out of all those who lately returned, at three elections, the six Conservative members for Dublin.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*July* 31, 1852.

'I shall not get any converts, for two reasons:—1. All the great parties would be against it; 2. Nine-tenths of the electors. Who wants the people to have time to think?

'The proposal to allow the Crown to name candidates at a given time, if, by the laches of the constituent body, none were named before, would have made an outcry, which would have been answered immediately by references to cases in which a public trust placed in private hands lapses if it be not executed: *e. g.* if the patron of a living do not present within six months, he loses his right, and the bishop gains the presentation.

'As to curry powder, which the poor Duke of Norfolk did recommend, as you state, I cannot make sonnets, but I have before now made epigrams; and when the burst of public ridicule came on the Duke—and all dukes—some of the newspapers defended

them by giving a list of dukes who had been in this and the other battle. Whereupon the following came into my head:—

> 'Oh! what than a duke can be prouder,
> Or who should be chanted out louder?
> The dukes all in battle have stood,
> And because they have been food for powder
> They offer us powder for food.

'Don't betray me.

'My plan would greatly diminish the quantity of oath-taking. *Now*, the candidates can have the bribery oath administered to all electors, and often do. At the same time I don't care for their having oaths. I only want the penalties of perjury, as in the declarations which have been substituted for oaths in affidavits.

'I got your seal, and the title, good for an advertisement. Your seal shows that between you and Adam come Scotchmen. How many generations of Irish ancestors have you?

'When you see the librarian of T. C. D., or any Irish bibliographer, professional or amateur, will you inquire whether there are any copies known to exist in Ireland of the editions, first and second, of Lucas Pacioli's *Summa di Arithmetica*, &c., the earliest printed work on algebra. An Italian, Prince Boncompagni, who is writing on this work, is collecting all the accounts he can of separate copies. Nor is it unnecessary, for in the same edition of the same book *three* different first pages have been discovered, and there may be more.

'The last day of letters prepaid in money. Henceforward *Quot epistolæ, tot capita reginæ*, but not the converse.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 3, 1852.

'For the satisfaction of an amateur astronomer of my acquaintance, I have spent some hours this morning on calculations, extremely easy in principle, respecting the intersections of the orbits of some of the asteroids. If $l_{m,n}$ and $L_{m,n}$ denote the latitude and longitude of the northern intersection of the great-circle orbits of an mth and an nth small planet, and if Ceres, Pallas, and Juno, be taken as the planets 1, 2, 3, I find, using the elements

in Herschel's large "Astronomy" (London, 1849, Appendix, page 647) the following numbers:—

$$l_{1,2} = 10°\,9'\,55''{\cdot}4\,; \quad L_{1,2} = 187°\,47'\ 0''{\cdot}7 \text{ (Ceres, Pallas).}$$
$$l_{1,3} = 8\ 17\ 15{\cdot}65\,; \quad L_{1,3} = 209\ 49\ 31{\cdot}0 \text{ (Ceres, Juno).}$$
$$l_{2,3} = 0\ 38\ 7{\cdot}1\,; \quad L_{2,3} = 173\ 39\ 12{\cdot}1 \text{ (Pallas, Juno).}$$

'I won't answer for *fractions* of seconds, but shall be surprised if, so far as the mere *trigonometry* is concerned, any one of these results shall be found to be a *whole second* wrong, as a *deduction* from the above-mentioned elements. Slight changes in those elements might sensibly affect the results; and I do not hazard any opinion as to the possible effects of perturbation, nor have I computed the radii vectores. But if my arithmetic be right, so far, it seems to be unfavourable to the notion of the asteroids being *fragments* of an old exploded planet. On the great circle which represents the heliocentric orbit of Juno, the northern intersections with the orbits of Ceres and Pallas seem to be distant from each other by an interval of more than thirty-six degrees. If I should find myself inclined, as a variety, to carry these little calculations any farther, do you think that the numerical results would be worth sending, to occupy a page, or less, of the *Philosophical Magazine*? [I suspect *not*.]

'You may, in the meantime, judge that I have no dislike to arithmetical or trigonometrical calculation, and that if I do not *work* more in that way than I do, it is merely because I imagine my time to be otherwise more usefully employed—an opinion which the elder Struve urged on me as his own, and as that of other continental astronomers, at Oxford in 1847, in terms more strong than I should willingly repeat; for though you may not believe it, I have *a little* modesty!

'*August* 4, 1852. This is my 47th birthday, and my younger son Archibald's 17th. Without any deliberate pre-arrangement, we were brought into the world by the same accoucheur, old Dr. Labatt (who was *not* the person *engaged* to attend my wife, but was sent out by him), within an *hour* or two of being *exactly* thirty years asunder! There was some talk of calling him "Halley," as the comet was just about to appear: at least his second name is Henry, of which you know the Falstaffian abridgment is Hal.

He is a tremendous book-worm; I consult his memory on all sorts of things. He is also a good boy.'

[The second sheet of this letter is unfortunately not extant—lost or destroyed. It probably contained Hamilton's account of his ancestry: *see* conclusion of De Morgan's letter of August 10. —R. P. G.]

From the SAME *to the* SAME.

'OBSERVATORY, *August* 8, 1852.

'I can spare for Mrs. De Morgan, if you think it worth while offering to her in my name, the enclosed lithograph of the original letter, in which Wolfe sent to a friend of his the well-known lines on the death of Sir John Moore.

'Sheet 2 x will be finished in a day or two, and then there will remain only a short Preface, about which I must really *consult* you.

'I wish you would *at once* give me, as freely as you please, any *general* hints about what you think would be prudent and proper on the occasion—even as to whether the first or the third person would be best to adopt—only remembering that I do not *promise* to do more than carefully to *weigh* any advice so given. Only part of a week remains to me; but I write quickly, when in the mood.

'P. S.—Lady H. and my daughter were saying, at breakfast this morning, that they should like to *see* De Morgan here.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*August* 9, 1852.

'Yours received—both the old and the new.

'The results you mention would be quite worth recording. I suspect the actual augmentation of the number of small planets, and the *possible* augmentation, have rather a tendency to destroy the old idea of a planet split to pieces. Looking at the nebular hypothesis—which will be very much looked at in the century coming—the idea strikes one of a *ring*, nearly thick enough for maintenance, broken by some accident. If so, these little animals

will swarm in a certain zodiac; or perhaps, if Saturn's ring be not solid, but composed of very many and close satellites, we have in the asteroids the rudiments of such a ring.

'I am at bibliography again, with the intention of giving something in the *Companion to the Almanack* next year. By way of a fair trial, I took down from my shelves the first four old books which presented themselves, binding myself in thought to make the trial, how much error and confusion would arise out of the four, either from the description others have given of them, or the description they give of themselves. I thought it probable that *four* books would yield *two* cases: they have yielded four, as you shall see in time.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*August* 10, 1852.

'My wife is very much obliged by the lithograph, which is very interesting. Now as to your points of difficulty.

'As to the first or third person, I, for one, *disgoze* the third person (old Cambridge verb active, for "repel with disgust.") To me

'Segnius irritant animum demissa per "ille,"
'Quam quæ sunt "ego" subjecta fideli.

'I disgoze also "we" from an author writing under his own signature. And our English is so entangled by want of genders and inflexions, that when "he," the author, and "he," the reader, get mixed up in a sentence, as they certainly will do, the clauses will change meanings, as Hamlet and Laertes did rapiers in the scuffle. No! No! keep I to take care of No. 1, and let "he" be the reader = "he or she," and let "it" be the book.

'Your preface should, I think, give a concise and well-dated historical account, with a reference in intelligible bibliography, to everything you have published. This, with a total absence of all apology or deprecation—in fact, an assumption of the right to invent quaternions, to write on them, and to print the book—may be joined to anything you please that is not actionable.

'I got your account of your ancestry: the following may try whether you have any Scottish feeling left.

[He then gives the famous *brutalité* of Dr. Parr in reply to Sir James Macintosh about O'Quigley, ending with the words: "He was an Irishman; he might have been a Scotchman."]

'What will an Irish Hamilton say to the last clause?'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *August* 11, 1852.

'This is my little daughter's twelfth birthday, and I hunted for your puzzle [square dissected into three squares] to amuse her; but the solutions, such as they are, have been made out by myself. Do you suppose that any third is possible? The red ink is a birthday present *from her*, which I found on my pillow, in a parcel with pens, &c., when I awoke pretty early this morning; *that* is always *her* notion of *her* birthday, that *she* is to make the presents. Her mamma found a pretty parasol.

'Are these your solutions of the puzzle? The pieces had been missing for some time, and only turned up to-day.'

From the SAME *to the* SAME.

'OBSERVATORY, *August* 13, 1852.

'You asked me lately whether I knew anything of Chasles' new work, *Traité de Géométrie Supérieure*. He has had the goodness to send me a copy, and I read this morning, honestly and fairly, the first six chapters *through*, to the end of page 80, not skipping anything, but, on the contrary, detecting several mistakes of the press, not after all very important, nor perhaps worth noticing at all.

'Besides *regularly* reading so far, I have skimmed much more, and even read with care some parts, up and down, near the end. I think I have now a *perfectly* clear notion of *homographic* figures, and of some other connected things. But what, as you may guess, more interested me just at present, was Chasles' view about imaginaries, and his application of it to *imaginary circles*, and *their* connexion with *real cones*. It seems all very good, but appears to have no relation to the quaternions.

'Since you say I must give *dates*, I must postpone publication for at least a *week* beyond next Monday; and if anyone happens

to speak to you about my book (London I know has other things to think and talk of), you may say, if you choose, that some slight causes of delay will prevent it from appearing *quite* so soon as was announced, but that it will almost certainly be out before the end of this month.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*August* 16, 1852.

'Yours received. I am just come in from Herne Bay, seven miles from Canterbury, where I have been reading novels for three days, and where I have left my family. I have no doubt your *two* solutions were the only ones possible. I was told by the inventor, H. Perigal, that there were only two.

'Never mind the waiting a day or two, or advertisements. Dates are of as much importance to an historian as to an Arab. The Arab, however, has to dry his; the historian's are as dry as possible from the outset.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 20, 1852.

'Without any peculiar call of conscience to do so, I have been reading sermons all this morning, an old theological essay of my own included, which was printed in the *Irish Ecclesiastical Journal* for (I think) May, 1842. You are not very likely ever to happen to meet that essay, or short Paper, on the Ascension—of which I could send you a written copy, only that it might look controversial; but the only controversial *hit* which it contained was, will you believe it, directed against an English clergyman, who afterwards came over here, and became my rector and friend—if a layman may speak of *his* rector—and who is now a bishop in England. I complained, and (I think) in conversation afterwards convinced him, that he had not been *orthodox enough*. If you ever read my Paper you will see that I am quite as Trinitarian as Athanasius, only I have a different mode of expressing myself. If you have any wish to know my sentiments, you must not assign *too much* importance to my having, in a letter of some months ago, attributed (I speak from memory) what many persons of my own Church

might consider *too little* importance to "hypostatical" questions. I go with the Catholic Church (that of England and Ireland included) on those points; but do not regard the *scholastic* aspect as the *vital* one. The influence on the heart and affections, rather than on the intellect, seems to me to be the important thing: and I am not sure that I shall not enclose a copy of that old Essay of my own, with perhaps a sonnet which dared to versify the *Te Deum*, as examples of the *manner* in which Trinitarian doctrines may be accepted without injury to charity. Whatever you may, in that case, think of *my* writings on such subjects, I feel quite *sure* that you *must* be pleased with a sermon, of which I think of sending you soon a printed copy, by the Rev. George Salmon—the Mathematician, whose works you know—preached at a recent Ordination held by the Archbishop of Dublin, and yet in which (I think) you could scarcely find a single *sentence* to object to. (Quite otherwise with *my* Essay—you would object to *every* sentence in it.)

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*August* 23, 1852.

'I have just finished sorting our friend Baily's correspondence. What a man! He kept and put into the rest, in alphabetical order, acceptances of invitations to dinner, apology from the candle man for the defects of the last batch, the printed request of the Assurance Office to know if the individual underwritten (who happened to be A. De M.) was of good health and sober habits, et multa *similiora*. I have, however, settled what shall be sent to Airy to be preserved at Greenwich, and what shall be destroyed. One or two letters from you on astronomical subjects are among the former. Your handwriting is diminishing as you grow older, and so I see is mine. We ought all to write a *round hand* copy now and then, or at least to **keep a very broad-edged pen, as I do, and use it sometimes, writing large enough to show the loops unblotted.**

'This job being finished, I can find time to turn to you and your orthodoxy.

'1. You say that you are as Trinitarian as Athanasius, and afterwards that you go with the Catholic Church. If so, you are

as Trinitarian as the Athanasian Creed, which is a great deal more Trinitarian than Athanasius.

'2. Going with the Catholic Church, you do *not* regard the scholastic aspect as the *vital one*. The Athanasian Creed does, and it *is* the creed of the C. C.

'3. In your versification of the *Te Deum* you Trinitarianize it. As it stands no Arian would object to it, provided he might take it literally, without any implications from other sources. I have often heard it asserted that the Lord's Prayer is Trinitarian.

'4. In your sunshine in the Cathedral I am totally puzzled. Is it Herschel, Forbes, and Hamilton, who are "in essence one; in name and office three"? If so, you would be called, if you mean the comparison to apply to the Trinity, *tritheistic*, by all the writers on the controversy. If you mean that *the Church* is in essence, &c., I do not understand. I am curious to know what you have read on the history of this controversy.

'Your "sermon," to which you prophesy the strongest opposition on my part, is the only thing against which I have nothing to say. I can dispute, on historical evidence, that Athanasius was an Athanasian. I can settle by its own creed what the Church of England professes. I can dispute the right of anyone to Trinitarianize old formulæ made, as I believe, before the *Athanasian* doctrine of the Trinity was in existence; and I can cry "heresy" against any one who understands the Trinity in unity as a correlative idea to that of the one notion man existing in three different men. But your "conjectural" account of the Ascension I am wholly without any means of either disputing or confirming.

'Finally, you are, what *very* many Churchmen are—Roman Catholics say, *most*—a *high Arian*, but you do not know it—say a high high-Arian; or, at least, your language bears that construction to a person who is so hackneyed in the terms of the controversy as myself. I do not think there is much real Athanasianism in the Church of England. But primary and secondary meanings of words are so mingled, that two persons who communicate on the subject need a *dictionary* of definitions—not a *chapter*.

'Here is an adroit play upon a word in its two meanings:—

'R. Robinson, a Baptist minister, well known in the last century, was in bad odour in his sect, for various heterodoxies; among others it was supposed that he did not admit any corporal

or personal existence to the Devil. A lady of his sect once said to him: "Mr. Robinson, is it really true that you do not believe in the Devil?" "Believe in the Devil, madam," said Robinson, "certainly not; I believe in God—don't you?"

'I believe I returned you my wife's best thanks for the lithograph of the monody on Sir John Moore. It is a very decisive piece of evidence.

'As you have copied for my amusement, I will copy for yours, that you may see what low practical jokes I can descend to. I found among Baily's papers a valentine, which I caused to be sent to him, in the handwriting of a fair Irish cousin of mine, a little while after the publication of the Flamsteed papers. I supposed he guessed from whom it came.'*

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 26, 1852.

'Mrs. Flamsteed's letter to Mr. Baily is exquisitely humorous; and I will not show it to anyone who (like Lady Hamilton, to whom I have *not* shown it) might be apt to consider it profane.

'The particular trinity which I had in view in my Ely sonnet was the sisterhood of the three Protestant Episcopal Churches of England, Scotland, and Ireland, represented, for the moment, by their three respective members—Herschel, Forbes, and myself. Rightly or wrongly, I took those three *Churches* as = one *Church*. That the three of us should happen to be kneeling together (quite close, in the Dean's pew, or seat, or whatever it is to be called), struck me as noticeable, especially as the sudden sunshine impressed whatever was poetical or excitable in myself at the moment; and falling in with other cathedral images and associations, led me to compose the sonnet in question, which, after all, I had no particular right to call a "trinitarian" one.

'It was composed (in 1845) without any pencil or paper, &c., while I paced the long cathedral walks after Divine service, Herschel and Lady Herschel having also remained in the edifice, and being engaged in examining some of the architectural points thereof. After some time we met, under the lofty "lantern";

[* See De Morgan's *Budget of Paradoxes*, p. 188. See also, *supra*, p. 44.]

and I recited to them my as yet unwritten sonnet of the day. After a while Herschel drew me, or, if you will have it so, *I* drew *him*, into a conversation about the quaternions, at Dean Peacock's residence, where we were guests together; and some gentleman, whose name I forget, said afterwards that he had heard and remembered so much as this of the dialogue, that I had ventured to express an opinion to the effect, that "a quaternion, or a function of a quaternion, *might* be the law of the universe." [Unpolarised *plus* polarised intensity = a function of time and space.] Next morning, as my bedroom adjoins Herschel's, and thin partitions did my madness from his great wit divide, I early heard what Burns might have called a "crooning"; and was not much surprised, though of course looking out for a *fan*, when, before we sat down to breakfast at the Deanery, Lady Herschel handed me, in her husband's name, and her own—for he had contrived to bring *her* into it—a sonnet of *his* to *me*, which, unless the spirit of egotism shall seize me with some unexpected strength, I have no notion of letting *you* see. You *did* return me the thanks of Mrs. De Morgan for the lithograph: I was glad that it gave pleasure.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*August* 30, 1852.

'Your sheet and proof received. Now tell me the history of the *facsimile* of Wolfe's monody. When was it done? Where is it referred to in the printed discussions on the authorship? There has been a good deal of reference to the subject in the "Notes and Queries"; but I have not seen the *facsimile* referred to. Who vouches for Wolfe's handwriting?

'With your letter came one from Peacock, who reports himself restored to health, and able to work "several hours" a-day. His edition of Young's works was nearly finished, and was burnt in the fire at Clowes's. It is now being reprinted, so a Peacock is a Phœnix.

'I never should have guessed the solution of the *three Churches*.

'In the first place they are not three. "The United Church of England and Ireland" is one Church—not in any sense two—by article 5 of the Act of Union they are "one Protestant Episco-

pal" Church. The Puseyites, in rejecting the word Protestant, have actually shown themselves ignorant of the legal name of their own Church. However, not to be guilty of the ferocity of quoting an Act of Parliament against a sonnet, what would the High Church English say if they knew you have brought the non-episcopal church of Scotland into communion with them—they, of whose churchmanship bishops, priests, and deacons, are the essence—sometimes the whole essence, for they frequently forget the laity altogether?

'Your commerce of sonnets (Herschel's and yours) is a curious revelation of the inner life of the mathematicians. Who would have believed it? Some day I hope you will send me that sonnet, in spite of your determination. Herschel is a man of poetical elements, as the American said.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 31, 1852.

'I suspect that, as I thought months ago, I must give up history—perhaps leaving it to you—at all events to some future writer, who *may* be myself; I am too *near* the subject just now.

'If you write soon, address to me at Belfast (British Association). Though resolved not to discuss theology with you, I still intend to send you Salmon's sermon.'

From the Same *to the* Same.

'Observatory, *September* 1, 1852.

'I must have been *very* obscure, although perhaps not more than usually so, in that Ely sonnet, and in my explanation of it. Professor (James) Forbes, the one who has experimented on heat, and observed the motion of the glaciers, is a Scottish *Episcopalian*, not a Presbyterian. I had no notion of including the "kirk" in my little trinity of Churches; I am far too much a churchman for *that*. What you say about the English and Irish Protestant Churches being one, by the Act of Union, is quite true; but since in common parlance they are treated as two, and in some practical respects are still distinct, I thought myself free in a sonnet to treat them as being such.

'When I was at Edinburgh, in 1850, I went to Bishop Terrot's

church and heard him preach; I also formed some acquaintance with him.* The Duchess of Kent attended another Protestant Episcopalian Church—or perhaps it is to be called chapel, although large—at the same time. I had been in the same beautiful city twice before—in 1834 and in 1827. In 1834 I attended, out of politeness, at a Presbyterian place of worship; in 1827, I am ashamed to say that I cannot remember whether I went to church at all. At Belfast, next Sunday, I have some expectation of hearing my own Archbishop (Whately) preach. He does so, now and then, in my own parish church of Castleknock, the same with which Swift was connected, if I have not wrongly caught up that story. Did you ever hear that I was served, about a year and a-half ago, with a "Monition" from the Archbishop's Court, enjoining me, as one who had been elected a churchwarden by my co-parishoners to remove certain emblems, especially a dove and a pelican, which were held by his Grace to be superstitious or unscriptural, from a large and handsome stained-glass window, which was, during my year of office, erected at the east end of the said parish church, at the expense of a pious widow? An inscription, in which she had brought God's name and her husband's somewhat too closely together, was also objected to; but, what had scarcely occurred to me as likely, the Archbishop was willing to leave *in* the deceased husband's name, if we struck *out* the name of God. He has, I believe, a theory, which perhaps you also hold, that it is Popish to regard decorations of houses of worship as contributing to the honour of God; and I counted myself fortunate that, under the greater scandal of the *window*, the present of a handsome *organ*, costing perhaps a couple of hundred pounds, and anonymously made by a nephew of my wife while I was a churchwarden, escaped without censure or remark. It was *rich* to see the frightened air of the apparitor, when he served me with the Monition! He had heard, I suppose, of an *un*ecclesiastical use of windows, but we were on the *ground floor* at the time; and besides, I was (and am) far too dutiful a son of the Church to be rude to my Archbishop's messenger. So I asked him to sit down, chatted about the forms

* Bishop Terrot has published, and no doubt discovered for himself, an interesting and curious case of Cotes's Theorem; but I am sorry to say that I have since found it to be anticipated by one of the Bernoullis.

of service, and told him that I had *expected* some such thing, which was the truth. In fact, I flatter myself that, though I had made some unsuccessful attempts to divest myself of the office of churchwarden (which the law of the land, I am told, does not suffer a churchman to decline), I was of some real *use* in the matter of the window, by acting as a sort of mediator between parties.

'My rector is a personal friend of the widow, a lady now resident in England, but whose husband is buried in *our** churchyard, and had given, somewhat hastily—for he did not even consult his own wife!—a permission to have the window erected. The Archbishop, somehow, became jealous—I won't say that he may not have conscientiously disapproved of things in which I could see no harm—he was my superior and teacher in the affair; but many of us fancied, that if he had been early enough *asked* he might perhaps have made no objection. My co-churchwarden was a still higher churchman than myself, and had been wishing to present our parish church with *another* painted window. The lady's feelings were to be soothed, and a chaplain's importance to be conciliated. Altogether there was a precious mess; and trouble enough I had; but I repeat that I think I was of *use*. The window stands, an emblem or two being removed, according to orders, and the *whole* inscription being boarded over. The lady thanked me for my pains, and the Archbishop, last winter, at a party of Lord Clarendon's, putting his hand on my shoulder, pronounced me an honest man!—*apropos* to something about Lord Rosse. What I consider especially fortunate is, that we all escaped the newspapers; for so much correspondence passed, between one lady and several gentlemen, including a prelate, chaplain, rector, two churchwardens, and even a lawyer—not counting the scribe of the "Monition"—that when it was all over, another nephew of my wife observed that a simple and obvious mode of effacing everything objected to in the window would have been to paste over it all the letters that had passed!'

* I shall *probably* be buried there. It would be a luxury, which I have little right to hope for, to be buried in the College vaults. If I have time to make up money for the purpose, I may perhaps arrange to be committed to earth with my father, mother, and sisters, in the graveyard of St. Mary's, in Dublin. But Castleknock would do very well. I am quite willing to imagine that the subject will bear to be deferred.

From A. De Morgan *to* Sir W. R. Hamilton.

'7 Camden-street, Camden Town,
'*September* 1, 1852.

I meant to have reminded you that printers have a villainous habit, still growing, of putting books a year forward, when the title-page is printed in the last months of any year. Priority will be damaged some of these days by this practice. Pray do not only make them put 1852 on the title, but state when the *printing* began. I can bear witness that you meant to be out at Christmas last, and that I knew it couldn't be.

'Remember delivery of MSS. to printer is *publication*, to say nothing of delivery of proofs and fair sheets to me.

'I like your determination not to discuss theology. You send paragraph or sonnet, and I send answer—Voilà la discussion commencée. As the proverb says, "The fray begins at the second blow." You mean that you withdraw the record after the defendant's answer, and issue a new writ, *e.g.* Salmon's sermon yet to come. Mem.—I *will* read it, even if it be of the nth order.

'Now *your* theology brought back into my head the old lines which were written on a Bible, at the time when private judgment began to develop itself—

'Hic liber est in quo quærit sua dogmata quisque,
Invenit et pariter dogmata quisque sua.

And then there came into my head a paraphrase

'One day at least in every week
The sects of every kind
Their doctrines here are sure to seek
And just as sure to find.

From all this you will gather that I have received the fair sheet, 2 Y I think.

'I hope your trip to Belfast will do you good. For myself, *solitude* is my relaxation. When my family go away to the sea, and all the people leave town, and I sort all the letters, and put everything to rights, I feel what I used to hear called an *agreeable lull*.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *September* 2, 1852.

'I wrote a long letter (two sheets of note-paper) to you yesterday, yet believe that I did not say a word about the lithograph of the ode on the death of Sir John Moore, by Charles Wolfe. It is very easy for me, however, to give you full satisfaction on that subject. The lithograph was published in 1841, in the *Proceedings* of the Royal Irish Academy, the original letter of Wolfe having been produced before that body on the 26th of April in that year, during my Presidency, but while my friend Humphrey Lloyd was occupying the chair in my absence. It was Dr. Anster, the Irish translator of *Faust*, who produced the letter or fragment, which had been found by Dr. Luby, now Senior Fellow of T.C.D., among the papers of a deceased brother of his own, who was a college friend of the writer and receiver of that letter. At this moment I speak literally "from book," namely, from Part V. of the *Proceedings* of the R. I. A., for the year 1840–41, where the lithograph is inserted, between pages 88 and 89. But as I am on cordial terms with Doctors Lloyd, Anster, and Luby (Luby, author of a *Trigonometry*, &c.), it would cost me nothing to make a little farther inquiry, if you could possibly think it worth while. Dr. Anster remarked to the Academy that it was not his object to treat seriously the "insane pretensions, now and then put forward in the newspapers for this person or the other," but rather to furnish, and place on record, an *authentic* copy of the ode, from the text of which some deviations have been made in print: such as the erroneous substitution of "suddenly" for "sullenly" firing. Wolfe was before my time, but I am acquainted with his biographer, Archdeacon Russell; and a deceased friend of mine, the Rev. Dr. Samuel O'Sullivan, who knew a vast deal about Irish life and society in a now past generation, often told me that *he* was *with* Wolfe, during a part of the time when the poetical afflatus was upon him, in connexion with the subject of the ode.

'I ought to be in Belfast now, and hope to be so to-morrow. My Preface is a horrid bore to me. There is so much that I wish to say, and so little time or even space for saying it, and I so much wish to do justice to other people, while I have so little hope of being able to do it. In short I think that I must just cut the

knot, by one desperate sword-stroke, and come forward as a discoverer, perhaps, but not as an historian.

'Your joke about the "dates" amused Lady Hamilton.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*September* 3, 1852.

'I understand you at last. Prop. It is legitimate, at least for poetry, to consider the trinity as analogous to three churches in three different countries. But there is an older proposition. You remember the story of the mathematician returning *Paradise Lost* with "What does it prove"? He might easily have been answered. It proves that it is impossible to introduce the Trinity into poetry without either Tritheism or Arianism. When Milton's recently-found book proved him to have been formally Arian, the learned world (at least the reviewing world) said: "We always thought so; look at *Paradise Lost*." They had no right to think so. *Paradise Lost* is but one of two things, either Arian or Tritheistic by poetical necessity.

'Now for the church window. I would bet five to one (in Os, they are my rouleaux) that Whately was right. Emblems *are* what they are *understood to mean*. So says common sense; so says the law. And the pelican is notoriously and distinctively *Roman*. The *organ* is not superstitious, because it suggests not more of one side than another—in fact suggests nothing except what the player makes it suggest for the time being.

'I do not hold with the Archbishop that "it is (distinctively) Popish to regard decorations as to the honour of God." It is also Greek and English, to different extents. I believe it to be superstitious (which Popish is perhaps a synonym for in your sentence; but there are Protestant superstitions which are not Popish); but if I were able, by holding up a finger, to abolish superstition and mysticism, I would not. I should as soon take the hot manure away from a plant before it was fit to draw its nourishment from the soil. Religion unmixed with superstition does not assimilate with the mental organs, and will not until a far higher state of cultivation is attained. But we must keep what approach we have made, and not fall back, and therefore the pelican must be abolished.

'Not only churchmen, but dissenters, are bound to serve as churchwardens, and dissenters not unfrequently do serve in England. And without going to church during service time.

'My notions about burial would shock a poet. If I had quite my own way in the matter, I should say, "Let the machine in which I have done duty be carried to those whose business it is to mend it while in action, that they may, by examination of it, become better qualified to mend other machines. If you want some of my remains to perform a ceremony over, take any pair of old breeches you find in my drawers. As soon as I have ceased to think—whether by that which thought leaving the body, or by thought undergoing a temporary annihilation—the remains of the animals which fed me, and the remains of the animals which covered me, only differ in this—that the former were worked up by a much more skilful workman than the latter; and therefore the former are worth preserving for examination, while the latter may be buried if you please." Now I will give you time to recover from the horror you will feel at this bit of religion without superstition.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Hoop Hill House, Lurgan,
'*September* 16, 1852.

'In the midst of packing for a second change of position (a practical provection—what would Donaldson of the Cratylus say?), I just write this line to acknowledge that I received at least two or three notes from you at Belfast, where I made a mathematical communication which the local newspapers characterised as "exclusively scientific," and, therefore, not to be reported. Perhaps from my next place of rest, near the beautiful bay of Carlingford—this place is near Lough Neagh—I may be able to write to you.'

From the Same *to the* Same.

'Castle House, Carlingford,
'*September* 17, 1852.

'The morning before I left the Observatory for Belfast, some of my children asked me at breakfast, as they had often done before, how the Quaternions were getting on. To which my answer was, *Hang* the Quaternions! (I assure you I had so much

propriety left as not to say D—— them!) But now, after some pretty complete diversion of thought, for a while, from a subject which had occupied me, perhaps too much, I am beginning to feel an interest in it again; and to-day, before and after a sail of three or four hours on the lovely bay (of Carlingford) which, when I raise my head, I see, I quite enjoyed reading some of my own articles on definite integrals in quaternions, while walking in an old friend's garden here—the friend whose guest I am. Several other acquaintances, in Carlingford and Rostrevor, claim a little of my time on this occasion of my visiting this neighbourhood, and I do not now expect, though anxious to get home, to start as early as Monday. Therefore, if you are inclined to write me a line—though I have not *yet* answered your letters to Belfast—please to direct it to Carlingford (the place of oyster celebrity, as it this moment comes into my head to remember). It is not unlikely that I may write to you again to-morrow, partly about the winding-up of my "publication" (in the *common* sense of that word; *yours*, I am sure, is the correct one), and partly about other things.'

From A. DeMorgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*September* 20, 1852.

'Just been reading extracts from an Irish journal on the Duke of Wellington, and his dislike (alleged) of his own country. Query emerges—given X – land, what constitutes an X – man. The Duke was of a descent English within the memory of man; settled in Ireland: his mother Irish: at what period begins complete Hibernicism?

'I thank *you* (as I suppose) for the Belfast newspaper. The meeting seems to have thriven.

'I see that Lord Rosse's name is among those quoted for the Chancellorship of Oxford. Surely he would not trouble himself with such a post. As soon as the Dons get a man who would not awe them, they would pull him to pieces with their feuds.

'This town is so empty that it supplies no news.

'I forget if I asked you before who I. Walton was. He was the Dublin opponent of Berkeley's *Analyst*, and published three tracts in the very year of the *Analyst*. I have a suspicion he was the son of old Isaac, but I cannot verify either way.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, near Dublin,
'*September* 25, 1852.

'This note is to telegraph my safe arrival here, after a little tour of three weeks in the north of Ireland, which has done my health and spirits so much good, that Lady Hamilton says I "ought to go very often to Carlingford." While there I wrote, and at Rostrevor (on the opposite side of the bay) corrected and sent off to London last Tuesday, a little Paper for the *Phil. Mag.*, of which I received, and have examined, a proof this morning.

'P. S.—I had written your name in a copy of Salmon's *sermon* before starting, and shall look for it presently in the De Morgan basket, but cannot stay to do so at this moment.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*September* 27, 1852.

'You and your Rostrevor note probably travelled to Dublin together, and the note and your Dublin note also travelled together, and arrived together this morning. So your want of strategics has cost you a penny. As you are come home in wild health and spirits you will probably not care, but put it down to travelling expenses.

'Your *basket* reminds me (did I tell you?) that I have had fifty little portfolios made for 3*d.* a-piece. I dare say you could get them in Dublin for 2*d.* Nothing but two sheets of thin pasteboard, 4to size, with three bits of book-covering cloth or paper-imitation (the young people could make them beautifully) pasted on, so as to open out wedgewise, but with parallel planes (this is important). Accordingly, I have arranged all my letters in alphabetical order. But H is a heavy letter even without you and your namesake; so you have a portfolio to yourself. You have no idea what a provocative to order and decency is the having a number of little receptacles ready.

'I have just been getting up (for *Phil. Mag.*) the early history of *infinitesimals* in England. Few of us know that Leibnitz was perfectly well known in England before the dispute, and that

Newton's first provocative to an imperfect publication was *ds* and infinitely small quantities paraded under his own eyes by an English writer (Craig), who lent him his MSS. to read.

'What of Quaternions? I too may ask; but I suppose you are really on eve of publication.

'Don't forget Isaac Walton of T. C. D., the *Irish* assailant of Berkeley; he is much unknown. Misspelt *Wilson* in Thomson's *History of Royal Society*. Mr. Salmon is on the spot, I think, to look at records. I will read his sermon as I said before.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *September* 29, 1852.

'You remind me of sending you Salmon's sermon: the pamphlet itself I had not forgotten; on the contrary, I took it out of your basket yesterday, and read it for my own satisfaction, pencilling, from memory, in the middle of page twenty, the word φυσιοῖ. Just now I have asked my boys, and their tutor, but without success, to find in the Greek Testament the passage referred to, which I *remembered* to run thus: ἡ γνωσις φυσιοι, ἡ δε αγαπη οικοδομει. I have just found it for myself, in 1 Cor. viii. 1. Perhaps, in this instance, my memory is more of the heart than of the head; for I remember the passage being quoted to me in conversation, considerably above twenty years ago, with Salmon's (probably received) interpretation, as potential, not indicative, by one whose blood yet cries from Irish earth against his murderers: the Rev. Mr. Whitty, who was stoned to death while returning from a work of mercy. I do not mean of proselytism, with which he had not much to do. I have not read Salmon's sermon on Temporal Blessings, but believe so far in National Sins and Judgments as to think (whatever my Archbishop may), that Irish miseries are not unconnected, judicially, with Irish murders. It will be a genuine satisfaction to me, if—though it is too good a thing almost to hope for—Fathers B—— and C—— shall be transported, for the murderous assault on the Queen's troops, which they encouraged at Six-Mile-Bridge. At present I have no sympathy to spare for the criminal assailants in that business, who met with so deserved a punishment.

'Will you believe, after this, that I do not even *wish all* the

popish priests to be hanged, or even banished? On the contrary, on giving, about a year ago, a Douay Bible (not Testament), which, by-the-way, cost me something not inconsiderable—much more than a good copy of the authorized version—to a Roman Catholic female in this house, I told her to consult her priest as to reading it: and was informed that he gave her perfect permission to do so. Indeed, on a hint conveyed through her, the said priest, Father Dungan—somewhat of the older and better school of Romish ecclesiastics—waited on me soon afterwards, and accepted a £1 for the *temporal* use of his poor parishioners; that stipulation being made by me, on the ground that I was a Protestant myself. "Well, Sir," said he, "perhaps you cannot help it!" His charity led him, as you see, to cloak me with "invincible ignorance." But perhaps I have told you all this before. If I do so a third time, let me know.

'At the time of the potato famine I refused to assist the same priest in some expressly popish enterprise; but offered him money for temporals, which he, but quite kindly and politely, declined. At that time I gave away money more indiscriminately than I now choose to do, and he pointed to a crowd of people who were waiting at my door. Still I fancy that the priests are now much poorer than they were a few years ago.

'I have heaps of things to say, about biquaternions, &c., &c.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*September* 29, 1852.

'Please to send me name and address of your printer. My publisher will, perhaps, have to ask him for an estimate for a work—not of mine. It is, in fact, Boole who is meditating typography on his mathematical logic, which is a very original thing, and, for power of thought, worthy to be printed by the printer of the Quaternions. As he has Hibernicised himself, it is fit he should have an Irish printer, and I rather suspect that the Dublin man prints more cheaply than the London ones.

'You are not, perhaps, aware of the check which there is upon employing distant printers. It is the difficulty of being quite sure that they do not take off more than the stipulated number of

copies, and sell the overplus for their own benefit. The thing has been done. Of course respectable men can be trusted not to do this: the difficulty is to know the respectability. But there is no fear in the case of mathematical logic. Could you contrive a check? I see none but the publisher furnishing the paper, and having it made for the work with a peculiar water-mark.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,

'*October* 1, 1852.

'I sent yesterday a missive for the name and address of your printer. Yours received in the evening. Salmon's argument that God intended religion as a means of developing the inquisitive faculties is, I think, very sound. But it follows that differences of opinion were intended, for a unanimous uninfallibility would be just as drowsy a dormitory as an infallible Church. Therefore you and I are to differ in opinion, *quod erat inveniendum*, or, whether or no, *quod fuit inventum*. Therefore also Salmon and I are to differ, which we do on one point. He thinks he may deduce something from the Epistle to Timothy containing no *esoteric* doctrines. I think not. An epistle was too public a thing in those days; and Paul must needs know that Timothy would have to show, and probably permit copy of, his letter.

'I agree with you in wishing that the fathers of the Six-Mile-Bridge controversy may be transported if the matter can be proved against them.

'All Catholics, *now*, introduce the salvo of "invincible ignorance." This must be a modern doctrine, surely, since it is clear that they did believe that all the heathens—even those whose ignorance was never assailed—were fated to everlasting perdition.

'On looking at a tract of Walton (about whom I have a standing inquiry *at* the Irish in general), I find in it an answer to a tract of *Berkeley*, of which I have never heard—a *third* tract in the *Analyst* controversy. I never heard of more than *two* of *his*. It is entitled *Reasons for not replying to Mr. Walton's full answer*. Surely T. C. Library must have *everything* of Berkeley.

'As for Ireland, trust to the *Exodus*—the best thing that ever

happened. I did in one sense forestall the spirit of Archbishop Whately's pun as recorded in the newspapers. I avowed my delight that the Irish had got into Exodus, because there had been a great deal too much Genesis.

'When you gave the pound to the priest *for temporal purposes*, how do you know that you did not set free *another* pound for spiritual ones? The only offer I ever made to subscribe to a R. C. fund was when a friend of mine (a new convert) informed me that a subscription was making to build Cardinal Wiseman a palace. I offered to be at the expense of painting the street door, provided I might choose the colour and the number. This offer was not accepted.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 3, 1852.

'I have not at this moment your last note beside me, but regret that I have let a couple of days pass without answering it, although so many others have been left much longer, and are even still, unanswered. You inquired the name and address of my printers, or printer. He remonstrated with me, about a year ago, for not using the latter form of grammar: being probably what is called a "warm" man, and *very* probably far better off than I am in the world; but at all events being jealous lest I should fancy him to have "partners," in a concern which by honest industry he has gradually made his *own*, namely, the press of this Dublin University. When I wish to be polite, I address him, by post, as follows:—

'"M. H. Gill, Esq., University Press Office, Dublin."

'You can judge already, from what you have seen of my sheets, respecting the fitness of his establishment to conduct scientific printing; and I know that he professes to be ready to print in most languages, and, with a little time for preparation, to promise to print in any. In short, he is ambitious of excellence in his own business; has printed for Salmon, Jellett, and others in Mathematics, and (I think) for Hincks, in those investigations respecting the strangely deciphered characters of the ancient East. With *him*, if I were again correcting the impression of a passage in Apollonius, referred to in a note to the Preface of my Essay on

Pure Time, I should not be obliged to *argue*, or insist, that it made some difference whether a Beta or a Theta were used in a Greek quotation.

'It may be a foolish shyness, but although I am much *better* acquainted with Boole than with you, as far as *talk* goes, for he took tea with me at my temporary rooms in Christ Church, Oxford, in 1847, along with Adams and (I think) Wm. Thomson, and has since met me (or I him) at a grand dinner of doctors in Dublin, whereas I think that I have only *met you once* in my life; yet still, the habit of correspondence makes so great a difference in the feeling of freedom, that I should, on the whole, prefer your merely sending to him the enclosed *slip*, to your letting him *see* this note, however little *private* in any important sense it can be.

'P. S.—The quotation was from *Euclid*, after all, and the word ἐκβαλλομένη, which the then printers of the R. I. A. persisted for some time in making ἐκϑαλλομένη; and on my going personally to remonstrate, gravely assured me that it was of no consequence!'

From the SAME *to the* SAME.

'OBSERVATORY, *October* 4, 1852.

'Another note has reached me this morning, and I find that I can answer your query about Walton, or at least about the tract of Berkeley which you mention, without consulting the Library of T. C. D., which, from distance, is nearly useless to me. Berkeley's "Reasons for not replying to Mr. Walton's Full Answer, in a Letter to P. T. P.," are printed at page 49 (to page 62) of the third and last volume of a work of which the following is a copy of the title-page:—"The Works of George Berkeley, D.D., late Bishop of Cloyne in Ireland. To which is added an Account of his Life; and several of his Letters to Thomas Prior, Esq., Dean Gervois, Mr. Pope, &c. In three volumes. London: printed by J. F. Dove, St. John's Square; for Richard Priestly, 143, High Holborn, 1820."

'The Tract is bitterly ironical, as you may judge from the two last sentences of its first section, or division. "But those, Sir, are not the reasons I shall assign for not replying to Mr. Walton's full answer. The true reason is, that he seems at bottom a facetious man, who under the colour of an opponent writes on my side of the question, and really believes no more than I do of Sir Isaac

Newton's doctrine about fluctions (*sic*), which he exposes, contradicts, and confutes, with great skill and humour, under the mask of a grave vindication." [The word "fluxions" seems to be elsewhere spelled in the usual way, throughout the Tract.] *My copy* of the work is *not mine* (like Berkeley, you know, I am an Irishman); it belongs to the Earl of Dunraven; but if you cannot otherwise get a sight of it, he would probably let me lend it to you. Of Walton himself I know nothing, except what may be collected from the two tracts of Berkeley: the "Appendix," and the "Reasons." About my *own* Appendix, more anon. Yours again.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*October* 5, 1852.

'I see you are on bibliographical P's and Q's with me, but you have forgotten to state whether your edition of Berkeley, which is quite new to me, is 4to, 8vo, folio, 12mo, or 18mo—or what. I will not borrow it, for I shall find it at the British Museum. I had Berkeley in *two* vols., 4to, London, 1784; but the idea of space which they communicated was so large that, as they had no mathematical works, I was obliged to sell them. I am very much obliged to you for the reference to this edition. I used to deal with Priestley, in Holborn, but I never saw this edition exposed in his shop.

'Your phrase, "my copy is not mine," is not a bull. It is perfectly good English to use the same word in two different senses in one sentence, particularly when there is usage. An English farmer said: "I did not get so much for these calves as I expected; and I never thought I should." To *expect* is said of the price demanded: "How much do you *expect—ask*—for these calves"? Incongruity of language is no bull, for it expresses meaning. But incongruity of ideas (as in the case of the Irishman who was pulling up the rope, and finding it did not finish, cried out that somebody had cut off the other end of it) is the genuine bull.

'The late Dr. Thomson of Glasgow (the chemist), always spelt "fluc*t*ions."

'I shall forward the printer's name to Boole. I shall be very

glad to see his work out, for he has, I think, got hold of the true connexion of algebra and logic.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *October* 8, 1852.

'The mere look of this sheet may serve to distinguish it from my usual notes, sufficiently to help your memory in case of your being disposed to read, at some indefinitely future period, what it may contain respecting some recent investigations of mine, without troubling yourself about them at present.'

[This is the commencement of a letter of 12 folio pages. The conclusion was posted on the 28th of October. It related to Gauche Polygons, a subject treated of by Hamilton in communications to the R. I. A. in 1849, 1850, 1853. See also *Lectures on Quaternions*, p. 309.]

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*October* 23, 1852.

'I am trying a fine pen with which to write in books. I think anyone would suppose I was a small thin man, to look at the results. I have received sheet 1, possibly to be returned. This index speaks of pages up to 719. I have not received beyond 704. Your unfinished letter is *not* a bore; when the commencement of the session is terminated, I intend to approfound it.

'I not only found the edition of Berkeley you speak of, but *another*, later, edited by G. N. Wright, London, 1843, two vols., 8vo. It is singular that two editions of Berkeley should have been so recent, and hardly anybody have heard of them. The more so, as Wright says some liberties were taken with Berkeley's text in the quarto edition.

'Having given the nibbler a fair trial, I now resume my ordinary pen. I shall send you, in a few days, a paper on the early history of infinitely small quantities in England. It is but a little specimen of the suppressions which national controversy gives rise to. From the moment when Newton declared against infinitesimals (1704), which till then he had exclusively used in fluxions, the English world agreed to suppose that they never had

been used here, and to forget the works in which they had been used. All these works are *now* absent from the Royal Society library, except Newton's *Principia*.

'A student of mine asked me to-day to give him a reason for a fact which I did not know was a fact, and do not yet. He says, that if a figure be anyhow divided, and the compartments differently coloured, so that figures with any portion of common boundary *line* are differently coloured—four colours may be wanted, but not more. Query cannot a necessity for five or more be invented? As far as I see at this moment, if four *ultimate* compartments have each boundary line in common with one of the others, three of them inclose the fourth, and prevent any fifth from connexion with it. If this be true, four colours will colour any possible map, without any necessity for colour meeting colour except at a point.

'Now, it does seem that drawing three compartments with common boundary, two and two, you cannot make a fourth take boundary from all, except by inclosing one. But it is tricky work, and I am not sure of all convolutions. What do you say? And has it, if true, been noticed? My pupil says he guessed it in colouring a map of England. The more I think of it, the more evident it seems. If you retort with some very simple case which makes me out a stupid animal, I think I must do as the Sphynx did. If this rule be true, the following proposition of logic follows:—

'If A, B, C, D, be four names, of which any two might be confounded by breaking down some wall of definition, then some one of the names must be a species of some name which includes nothing external to the other three.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 26, 1852.

'I am not likely to attempt your "quaternion of colours" very soon.

'The difficulty about that horrid "Preface" of mine is the *historical* one. If *you* were in the humour, and found time, to send me any sketch of your own, of the history of "Double Algebra," or even of the history of your *own* extensions and improvements

thereof, I should be only too happy to insert such sketch in an Appendix. But that is probably too good a thing to hope for. *Some time or other* I trust that you *will* publish some historical account of our friend $\sqrt{-1}$, going back, by all means, to the Hindoos—about whom I know less now than when I was a boy. Meanwhile I shall give you one little bit of modern confession or record on the subject, which naturally interests myself.

'In page xiv of the Preface (Vorrede) to Grassmann's *Ausdehnungslehre*, a *very* original work, published (it appears) at Leipzig, in 1844—Verlag von Otto Wigand—which work, if any, the Germans, if they think *me* worth noticing, will perhaps set up in rivalship with mine, but which I did not see till long after my own views were formed and published. In that page xiv Grassmann says, under date "Stettin der 28, Juni 1844," . . . "Eben so nun zeigte sich auch umgekehrt, wie man vermittelst der so gefundenen Bedeutung des Imaginären auch die Gesetze der Analyse innerhalb der Ebene ableiten kann, hingegen ist es nicht mehr möglich, vermittelst des Imaginären auch die Gesetze *für den Raum* abzuleiten. Auch stellen sich überhaupt der Betrachtung der *Winkel im Raume* Schwierigkeiten* entgegen, zu deren allseitiger Lösung mir noch nicht hinreichende Musse geworden ist."

'The underpointings are my own. But I think that I *must* cite this passage, because Grassmann has *a* non-commutative principle of multiplication of his own, although the foregoing passage (dated June, 1844) seems to *prove* that he had not *then* (nor, so far as I have learned, *since*) the *notion*, much less the *laws* of the quaternion, which I communicated to the R. I. A. in October and November, 1843, and of which an outline was *printed* in the *Philosophical Magazine* for July, 1844.'

* I did not consider myself to have so much as *begun* the *geometrical* theory of the Quaternions—for an algebraical (or metaphysical) theory of *sets* had been long previously in my head—till I had (as I thought, and still think) *conquered* those difficulties for *angles in space*, which Grassmann avows to have been inextricably perplexing *him* when "publishing" several months later, in 1844.

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*October* 29, 1852.

'To write anything worthy of your book at the beginning of a Session is not at all on the cards for me. Moreover, I should think you might well content yourself with a well-dated account of your own doings, and of the analogous ones, so far as they touch triplets or quaternions. The doublets you might assume. You would thus enable others to secure your own rights, and Grass-mann (quere, is his Christian name Nebuchadnezzar?) would fall at once into his proper place. The thing most requisite is a faithful account of what *you had seen* up to publication—I mean not a full account of all the books, but general notice.

'*Ex. gr.* when I wrote my Logic I gave an account, in general terms, of all I knew of any attempt to deal with the syllogism, otherwise than Aristotelically. From this it appears that I knew of *one* work of Ploucquet. Your Scottish synonyme, in his recent work, finding himself unable to refer to his charge of pillaging himself, finds out, or thinks he finds out, some of my ideas in *other* works of Ploucquet, which he mentions, and thereupon charges me with pillaging Ploucquet, whose *writings* (the man uses the plural) he says, I admit I had read. Now I am safe, and the charge is unworthy of any notice, for all who believe me commonly honest (and argument from me will be of no use with others) will see that I knew of but *one* work of Ploucquet, and that one not one of those which Sir W. H. cites as containing the matter in question.

'By the way, when you send me German, clarify it into English. For I belong to the school which reads German with a dictionary, and guesses the syntax; or, if it be mathematics, makes the equation translate the paragraph immediately preceding it. If you cite the passage, translate it.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 38, 1852.

'At last I have fairly dashed into my Preface-writing, having decided to continue, although as briefly as I can, a sketch which I began before I went to Belfast, of the progress of *my own* thoughts

on the whole subject, and especially to give a very abridged account of my old view of Pure Time, taking great pains, however, to avoid your old (and not ill-natured) charge of "dogmatism," by not pretending that mine is the *only* view which can be taken of the matter. As it really was the parent of the quaternions, so I have, over and over again, returned to the opinion, resisted through unwillingness to seem to insist too much on my own old speculations, that some general knowledge of the *point of view*, from which I looked at algebra long ago, would be a really useful *preparation* for a student of my later speculations on geometry. He would thereby see how *natural* it was for me to use, in the latter science, some notations which at first seem strange; and other advantages, I think, would follow.

'You may keep the unrevised half-sheet of *Contents;* but I wish you to return the enclosed slips of an *Appendix* (C), which will be found somewhat *stiff* (I suspect) by readers for some time to come. It records, in a condensed form, the leading steps of a long analysis; and the *condensation* has cost me more trouble, I think, than the original investigation did. On the same principle, the composition of the Table of Contents was perhaps the most laborious part of my work.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*November* 8, 1852.

'I got your note, and I think I got from you a newspaper to-day, about the Historical Society, for which thanks. I am glad to hear that you mean to give your *own* history at least, and that is all which can be *claimed*. I am up to my ears in series and mean values. I believe I have put the subject on the same footing as any question of limits. You take the *Philosophical Magazine*, I think. If you do, it is not worth while to bother you with separate copies of Papers in it, unless you bind tracts methodically.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 16, 1852.

'My manuscript, for slips five and six, which is already in the compositor's hands, begins with the word "triplets"; but a note

has been appended to that word, viz. :—"These remarks on triplets are now for the first time published." I think that they will interest *you*, as showing how far I had gone in February, 1835.'

From the SAME *to the* SAME.

'OBSERVATORY, *November* 18, 1852.*

'You need not trouble yourself to return the enclosed slip. I shall ask the printers to send me another copy.

'When I get slip six, and forward you a copy of it, you will find some equations transcribed from a manuscript-book of February, 1835, respecting the precise date of which it is impossible for me to have any doubt whatever. Even the *place* where I wrote the investigation is recorded in that old manuscript; it was Kilboy, the seat of Lord Dunalley, near Nenagh, who is not distantly connected with my wife's family, and who, although he has never had occasion or opportunity to show more than civility to her or me, exhibited in former years his sense of relationship by very substantial proofs, such as purchasing commissions in the army for more than one brother of hers—since dead. If it were needful, or becoming, I could attest in a court of justice the genuineness and date of the manuscript-book from which I have quoted, as having had nothing added to it since the month already mentioned. But I hope that few if any persons will think me such a *fool*, as to set up, at this time of day, any *claim* grounded on that manuscript, especially as against *you*. That would be too ridiculous; and though I can bear ridicule, I do not court it. As soon as your Paper on Triple Algebra (of which you sent me a proof-sheet) was published, I felt that the time for gracefully claiming the possession of triplets was lost to me. Accordingly you may have noticed that I have been since extremely guarded and cautious in my printed allusions to the subject. See, for instance, the last page of my quarto Essay (*First Series of Researches on Quaternions*), published in 1847. But it seems to me that if I now put on record some old conceptions and results of my own, to which I have more than once in print alluded, even so long ago as the very year 1835,

* The *date* reminds me how differently you are all engaged to-day in London. I have just been reading D'Israeli's speech respecting the Duke of Wellington, and have admired it, and sympathised with the nation.

in which chiefly the investigations were conducted, I cannot be supposed to wish to appropriate to myself any part of the credit which has been justly acquired by you, to whom those investigations were (except, perhaps, by name) unknown. And if I do *not now* publish some sketch of those old things of mine, it would be rashly sanguine in me to hope that it will *ever* be published. This is my *only* opportunity, so far as I can hope, and is one purchased, probably, by an inconvenient outlay of money, which I am not likely to repeat.

'But it is scarcely fair to seem to ask for advice on a point, on which, after all, I must act on my own responsibility.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*November* [20], 1852.

'I have got all the slips of preface up to six, just received, and have read them. You are quite right in resuscitating 1835. As to me, or anybody else, there is no occasion for reserve. I remember, as I have mentioned to you, that before I wrote on triple algebra in detail, I announced in one of my Papers that an algebra of those dimensions would require symbols ω and Ω, such that $a + b\omega + c\Omega = 0$ means $a = 0$, $b = 0$, $c = 0$. You answered the receipt of that Paper with—as to the above announcement—words of agreement existing, as opposed to words of acceptance.

'I did not go to the Duke's funeral. My wife and daughter did, and were not over-struck, except with the soldiery. I have hardly met with a person who was much struck. The car I have heard pronounced a cross between a locomotive and a fire-engine. The whole thing was so unlike the Duke, that the incongruity seems to strike most people. A procession of carriages cannot be made striking; I sincerely hope it will be the last carriage procession ever attempted. The banners, heralds, &c., are not even voted trumpery—nobody says a word about them. Except hearing of a little child who took Garter King-at-Arms for a beef-eater, I have not heard that great officer of state mentioned. So much for 1852 treated to a taste of 1552. If I were set to teach arithmetic to G. K.-at-A., I would make him write 1852 – 1552 = 300 as the first instance in definition of –. And if I were set to teach him Latin, I would make him construe "Omne ignotum pro magni-

fico" thus: "Talk of the grandeur of a procession to those who stay away."

'You could invest it all with a poetic halo, and forget that the light was all of your own contributing; but I am obliged to look for all that suggestion from without.

'So no more heterodoxy at present from yours truly.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *November* 24, 1852.

'It would have been very unlike your character, if you *had* disliked my "resuscitating" those old triplets of mine. But I have suffered so much annoyance, although not lately, from sensitiveness of other people to their imagined rights, that I have been made sensitive, or over-cautious, on the subject. If, by any good fortune for me, you have preserved that first letter of mine to you to which you have alluded, I shall be curious to know whether it does or does not contain the remark that if my views about time were correct, there *must* be to be found, not merely *triplets*, but "polyplets."* I am certain that such a remark was *in my head* at the time of my writing to acknowledge your first Paper on the Foundation of Algebra, and quoting Kant. Indeed I have reason to be *sure* that I *wrote* to you to that effect: but what I am *not* certain about is, whether I *sent* what I had written. For although I have not been a particularly timid correspondent, especially with you, it has several times happened that if a thing were not at once sent off, I thought it too free, or too restrained, or too something-or-other, to be done anything with, except being put into the fire.

'After all, there is very little in common between your Paper on Triple Algebra and my old investigations respecting triplets. The general *suggestion* of "the idea of inventing a distinct system of unit-symbols, and investigating or assigning relations which define their mode of action on each other," you very distinctly and honourably referred to the publication of my Paper on Quaternions in the *Philosophical Magazine* for July, 1844. You seem to suppose that I "passed over Triple Algebra altogether, on the supposition that the modulus, if any, of $a\xi + b\eta + c\zeta$ must be $\sqrt{(a^2+b^2+c^2)}$." It is true that I *wished* for such a modulus, as a child might wish

* *Supra*, vol. ii., p. 343.

for the moon; but you had probably never seen, or had forgotten, the closing sentence of my *published* Essay of 1835; and you are *now* aware that in the unpublished triplet system of that year which least dissatisfied me, I had *three real and linear moduli*, connected with a system of three rectangular lines.'

From the SAME *to the* SAME.

'OBSERVATORY, *November* 25, 1852.

'I enclose an old scrap, which I lit on just now among my papers. It seems to have been written as a hint—not used, I fancy—for a letter to you, on the occasion of your sending me the proof-sheet (or sheets) of your Paper on Triple Algebra. I really do not see how my old investigations on triplets *could* have assisted you in drawing up that Paper, *even if* they had been known to you, which (beyond, perhaps, the *name*) I *know*, that they were NOT. But for that very reason, the *obvious independence* of the two trains of calculation, except so far as relates to the mere *notion* of "*inventing* a distinct system of unit-symbols," which you were careful to assign to me, my old things on the subject are still *as good as new*, and may (as you seem to agree with me in thinking) be not merely without impropriety, but with some sort of utility, or interest, published. I cannot at present assert, and do not at present believe, that I had *anticipated the discovery of any one* of your *particular* triple systems. And, on the other hand, such examination as I have yet given to your Paper on Triple Algebra does not enable me to see that you have *forestalled the publication* of *any one* of those *particular* systems which interested me in February, 1835. Therefore in now publishing some *sketch* of my investigations of that date—and trouble enough it has given me to renew lately my own understanding of those old calculations, bristling all over as they do with symbols of the form 1_{fgh}, and not entirely free from $\sqrt{-1}$—I hope that, on the one hand, I am making some little contribution to *science* itself, and not merely to the literature or *history* of science; and on the other hand, am not expecting any curious *coincidence* of invention to be believed on my own testimony. The *coincidence* does not exist. You acknowledged a *suggestion*, and worked it out in your own way, as of course you had a right to do.

'One fundamental difference between your mode and mine of working out the notion of triplets consists in your assumption, thus expressed: "Also let η and ζ be interchangeable, and related in the same manner to ζ." Thus you made η^2 mean $a\xi + b\eta + c\zeta$, and ζ^2 mean $a\xi + c\eta + b\zeta$. If, in *other* respects, I had been using your notation, I should have written $\zeta^2 = a'\xi + c'\eta + b'\zeta$. *So far*, my method was *more general;* but, on the other hand, I was accustomed to assume the symbolic equation $X_1 = 1$—so I then wrote it—not as *necessary*, indeed, but as one eminently *natural*, in the view which I took of the subject. This answered to your *sometimes* treating your symbol ξ as "inoperative," and, *so far*, limited me within the conditions of your *case* B. But, for the reason lately mentioned, my *particular systems* were quite distinct from yours; and even now their reference to rectangular lines may have some interest.*

'I know that I have not destroyed, and hope that I shall be able to find, an old letter of John Graves, in which he compared my "gradual ascent" from singles to couples, from couples to triplets, and generally from *set* to *set*, to the ἐντελέχεια of Aristotle (I *think*, in 1835).

'As to the mere *notion* of *triplets*, I find memoranda of mine, so old as 1830, which I have no intention of *publishing* now, but may perhaps *communicate* to *you*.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 15, 1852.

[In acknowledgment of a proof-slip of page (41) *Lectures on Quaternions*, to which Hamilton had appended a manuscript note upon the words 'alluded to,' referring to De Morgan's recognition of Hamilton's previous dealing with triplets: 'Your first note to me, in May, 1841. . . . You invited me in it to tell you anything about those researches of mine on triplets to which I had in print alluded. You said something like this "time-triplets or space-triplets, I don't care which."'—*Supra*, p. 245.]

* *Supra*, pp. 251, 252.

'I supposed you received mine, containing certain extracts from your old letters. I have received your slips. I long to see it all together, for the slips are properly so-called; they slip objectively into masses of other papers, and subjectively, the contents slip so that with each slip it is ascertained that the former has slipped.

'You are evidently in some internal agitation of possibility of a priority controversy. You may make yourself very easy: you are putting on record as I recommended you, and nothing can refute the idiosyncratic character of your old investigations. They are yourself all over. All men owe to others, and a phrase in a book of metaphysics may suggest a step in mathematics. Grass has no right to raise a priority contest against anything that is published as mutton, if it be really mutton when published. Now your grass has been cropped equally and fairly among the dead and living, and your mutton is genuine.

'N. B.—It must hang awhile before many people will have the full relish for it.

'I am deep in the idea of form and matter, as transferred from logic to algebra. I only write this to beg that, if it seem good and I am not too late, you will omit or qualify all phrases in which you hint anything as to the possibility of your priority being contested, as at the bottom of slip eleven.

'So putting you to think of this, I remain, &c.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 18, 1852.

'I did receive, and was obliged by, your letter containing extracts from old letters of mine. If time allows, I shall send you a copy of your *first* note to me.

'I am conscious of being too fidgetty and nervous on any subject of anticipated controversy, for like Croaker, in Goldsmith's Good-natured Man, when anything like actual attack or criticism occurs, I find that I have fretted away all my vexation beforehand.

'At Manchester, in 1842, if I may trust my memory, Jacobi (the great one of that name) commented publicly on my Memoir on Fluctuating Functions; of which, if you have not received, or have mislaid, a copy, it is not too late for me to supply the defect. In that Memoir of mine, after it had been printed, I perceived

resemblances to passages of your Differential and Integral Calculus, which struck me sufficiently to make me say, at that Manchester Meeting, that I hoped no good-natured "friend" would make a quarrel out of it. I meant that I trusted no one would get up a quarrel between *you* and *me* about it; but poor Mac Cullagh, who was present, contrived, in some incomprehensible way, to take me as controverting, at least potentially, with himself.

'Since I have mentioned him, I shall take this opportunity to send, if I can soon lay my hand on it, *his* first note to me. I think that I have preserved another note of his, enclosing an attempt of Walker to trisect the angle! sent after his death to me. Will you believe it? Fears of the brave, and follies of the wise. I took some trouble at the time with that attempted solution, as I have done with too many others, not having, all the while the faintest shadow of belief in them, but wishing to do justice to the persons, and if possible convince those who were living of their error. And I am happy to say that in two instances out of many I succeeded, and was thereby useful. One case was that of a clergyman of the Established Church; his education had not prevented him from falling into error, but enabled him to see it, when pointed out. The other case was that of a lad, about to enter College here, who was brought to me by an aunt of his. He quite admitted his mistake, when shown him, and afterwards distinguished himself in this University.

'After all, are you *sure* that it is impossible to trisect the angle by Euclid? I have not to lament a single hour thrown away on the attempt, but fancy that it is rather a tact, a feeling, than a proof, which makes us think that the thing cannot be done. No doubt we are influenced by the cubic form of the algebraic equation. But would Gauss's inscription of the regular polygon of seventeen sides have seemed, a century ago, much less an impossible thing, by line and circle?'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 24, 1852.

'The excuse about fretting beforehand is not available unless you can show that a larger quantity of it than you can obtain *à posteriori* is necessary to your moral health; for, you fret not only

for the things which do happen, but for those which do not. You should not show any such symptoms. Consider what a reflection it is upon Lady Hamilton, that you cannot get the little incidental troubles of a household in number or magnitude sufficient to keep you from fretting over imaginary priority quarrels yet to come, or anything else that may—not does—turn up. Think with what a force such resolutions as the following, from the Mathematical Section of the British Association, would come:—

'1. That a tendency to show fear of falling into discussions about priority increases the probability of their actual occurrence, the same being against the peace of science, its progress and dignity.

'2. That it appears from the premises (duly set forth, of course) that Lady Hamilton, no doubt with the best intentions, but with a slight failure of judgment, makes her husband too comfortable, so that he is obliged to invent his own vexations, his powers of invention most readily exercising themselves in mathematical science and things thereunto appertaining.

'3. That the members of this section, from their several experiences, take the liberty of recommending to her ladyship's especial consideration a practice which, by itself, they know to give an amount of discomposure enough for the ordinary uses of life; namely, that of causing Sir W. H.'s books and papers to be arranged, once at least in each week, by a servant who is decidedly under the impression that written papers are to be sorted by size, and that all the much-disputed art of cataloguing a library consists in subdividing works into those which have VOL. at the back, those which have TOM., and those which have neither.

'I return M'Cullagh's letters with thanks. I have the Fluctuating Functions, and out of it, mostly, have manufactured an indisputable proof of Fourier's Theorem. Why did you not generalize the symbol sin x and cos x, instead of P_x-ing and Q_x-ing them?

'As to the trisection of the angle, Gauss's discovery increases my disbelief in its possibility. When $x^{17} = 1$ is separated into quadratic factors, we see how a construction by circles may tell. But, it being granted that $ax^3 + bx^2 + cx + d$ is *not* separable into a real quadratic and a linear factor, I cannot imagine how a set of intersections of circles can possibly give no more nor less than three distinct points.

'I never convinced a squarer or trisector who proceeded on mathematical principles—*permanently*. I once persuaded a R. C. priest, who came from South America with a square circle in his pocket to get the reward, that he had better learn geometry first. The argument which seemed to weigh most with him was that the only known instance of a cure was one effected on a Jesuit mentioned by Montucla. But he relapsed, for his book was advertised (I mean *my* priest's book) a few days afterwards.

'I have just put my ending to a Paper on the Forms of Algebra. Now I will tell you a story—

'Let the curve be $\int_0^\infty \frac{\sin xv}{v}\,dv$.*

'Let ∞ be the reciprocal of a certain circle Q, $\infty = \frac{1}{0}$.

'As long as x is $> 0^\alpha$, no matter ($\alpha < 1$) how small α is, the point which describes the curve moves from . . . B to A. While α moves towards 1, A is turning the evanescent corner, and never takes permission to consider itself off the line AB, and on AC, until α is 1. Then while x is changing through all the varieties of $m \times 0$, m positive, from $m = \infty$ down to $m = 0$, the point moves from A to O. At O it remains without moving a bit until α has got up to ∞, 0^∞ being a zero of zeros; the negative 0's, or powers of 0 with negative multipliers, then come into action, and the part OCD is described by reverse steps.

'I have several other figments of more importance; but I remember the name of that unfortunate French minister M. Troplong, and must avoid acquiring it for myself.

'The wishes of the season to you and all your family.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 27, 1852.

'Your note arrived this morning, and what I read of it to Lady Hamilton amused her much. I think of striking out a passage or

* [The diagram, here inserted in the manuscript, consists of a pair of rectangular axes meeting in the origin O: from it $OA = \frac{\pi}{2}$, and $OC = -\frac{\pi}{2}$ are measured along the axis of ordinates. Then through A is drawn the indefinite line AB parallel to the positive axis of abscissæ, and through C, CD parallel to the negative axis of abscissæ.]

two, which looked superfluously cautious. I am just sending off the early slips of Preface to be *paged*, in half sheets or whole sheets, I leave the printers to decide; and I trust to *sign* finally what remains to be written before the end of the present year, so that the *composition* may not extend into 1853.

'Yours in haste (as usual). I hope to write soon again.'

From the SAME *to the* SAME.

'OBSERVATORY, *January* 8, 1853.

'. . . The fourth half sheet I do not like to sign, without its containing a note which is to touch, though slightly, on the History of Double Algebra—not including for the moment your own improvements about Logometers, &c. It seems to me that the true author of Double Algebra is Argand, not Buée—but if you take the trouble to refer me to any paragraph or paragraphs in Buée's Paper, of which I possess a carefully collated copy, *partly* written out by myself from the *Philosophical Transactions* for 1806, which paragraphs, in your opinion, contain or shadow out the rule for multiplication of lines within the plane, I shall, in justice to him, and prudence to myself, examine them.

'On *this* point "an early answer will oblige," because my printing requires it, or at least will not suffer me to pause too long, although I so often leave unanswered, or long delay to answer, important things in your letters, not counting subjects of controversy.

'About definite integrals, and fluctuating functions, I by no means despair of resuming those subjects, or at least examining my own manuscripts; but shall probably turn in to some labour in practical astronomy, when I get my book off my mind. Meantime I have been looking after the Observatory of late somewhat more than I had done for a good while.

'"Masses"—I repeat the word—of observations have accumulated here, and *something* has been done towards reducing them, but far too little.

'But I have not another moment now.

'Many happy new years!'

From the SAME *to the* SAME.

'OBSERVATORY, *January* 26, 1853.

'I suppose that I and others must just submit to whatever you may at any time choose to make your own *definition* of Double Algebra; and as you once were pleased to admit that my old doctrine of Algebraic Couples was *a* sort of Double Algebra, so I must grant that *something double* was involved in Buée's Paper; for he did, no doubt, in his No. 35 for example, represent a directed diagonal of a square by the expression $\sqrt{2}\,(\cos 45° + \sin 45°\sqrt{-1})$, or by $1+\sqrt{-1}$. But turn to your own book on *Trigonometry and Double Algebra*, pp. 118, 119. Don't you see how naturally and inevitably *you* associate the construction of addition and multiplication of lines within one plane, as going together to make up *one* system, about which you judged (I think rightly) that it was worth while to write a book? How nicely do the figures match, at opening of the pages! Not disputing the value and importance of your own improvements, as to logometers, &c., I ask, Should you have cared to write about Double Algebra, if it only involved addition and subtraction? Buée had *those* operations, in 1806, or rather in 1805, as I concede, though scarcely in a more developed state than Wallis in 1685. Just turn to Wallis's geometrical construction for the roots of the quadratic equation,

$$aa \mp ba + æ = 0,$$

in chap. LXVIII. of his *Treatise of Algebra* . . . . London . . . . MDCLXXXV.," from which I have at present only a few passages at hand, transcribed by myself in 1846. Does he not seem to have regarded* the straight [he spells it streight] line ACa as being the *sum*, or at the very least as being *analogous* to the sum, of the *two inclined lines*, AB, and Ba, by which two lines he constructs the *two roots*, a, of the quadratic, their *algebraic* sum being $= b = ACa$? And did he not regard the line BC as representing the *semi-difference* of the same two *directed* lines AB and Ba? (His angle PBC is right, for the case of imaginary roots, or for $PC = \sqrt{æ}$ being "bigger" than PB, which latter line $= AC = Ca = \frac{b}{2}$. His phrase-

* In Wallis's figure AB and Ba are any two inclined lines, C is the middle point of Aa, and BP is at right angles to BC, and equal to AC or Ca.

ology, "what we call *Real*," is very remarkable.) I think Buée's construction of imaginary roots more elegant; but can you point to any passage in Buée's Paper (120 years later than Wallis's Treatise) which contains even the *germ* of the method illustrated by the figure in page 119 of your lately cited book? And can you explain away Buée's own paragraph 46, in which, when he proceeds to state *in what sense he* regards *his* geometrically constructed roots, AE, EB, of Carnot's quadratic, $x(a-x)=\frac{a^2}{2}$, as having the required *product*, $\frac{a^2}{2}$, he *expressly* and *formally excludes* the consideration of *position* (or of direction of the factor-lines), and *expressly* and *formally limits* the proposed *multiplication* to that of the *arithmetical values*? I have no more of *personal* interest in the matter than you can have in a question about Newton and Leibnitz; but I wish that justice should be done, and wish you to help me in doing it (namely, to Argand, who was, in my opinion, the *inventor* of Warren's rule of multiplication).'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*January* 27, 1853.

Seriatim. *I* don't claim to define Double Algebra, or to define anything except my own appetite at dinner, and then I try to satisfy the definition. I found it—double algebra I mean—as a system (incomplete) first in Warren, who was obliged to do without a general A^B. I found portions of it in preceding writers.

As to Buée, I agree with Peacock, p. 228 (*Report*, &c.). Buée had no completely formed double algebra, and yet he was the first formal maintainer of geometrical interpretation of $\sqrt{-1}$. But in what did he go beyond Wallis? You answer this yourself. You think Wallis "seems to have regarded" C as $A+B$. No, you, with your lights, know that you could not have done what Wallis did without having more in your mind. It was long before I got at the maxim, that in reading an old mathematician you will not read his riddle unless you plough with his heifer; you must see with his light, if you want to know how much he saw. Now we know that Wallis does not express any perception of more than "geometrical affections answering to the resolutions of such qua-

dratick equations . . .” And his conclusion is, that the point required lies *out* of the line of positive and negative.

‘He made a commencement of geometrical *interpretation, ex post facto*, as to the result. Buée made a commencement of algebraic explanation as a basis of continual operation. Wallis has not a dream of reasoning from $a+b\sqrt{-1}$ as an introduction into data.

Buée was imperfect altogether as to multiplication. I did not know (it is long since I have *seen* Argand) that he was the inventor of the rule of multiplication. Is it really distinctly so? Peacock did not see it. If so, he is at a stage of double algebra below Warren, who, though he did not explain A^B, or even $\epsilon^{\theta\sqrt{-1}}$, yet supplied their places by new creatures of definition derived from the roots of unity.

‘You say he is in your *opinion* the inventor of this rule; as against Warren, he must be so if he had it at all. But how comes it to be matter of *opinion*? Are you reading him by your own light again?

‘I am always afraid of *virtual* discoveries. Men usually tell all they definitely see. What we gather we frequently gather by our own act. There are plenty of places in which one man has seen in one place of his book that every A is B, and in another that every B is C, and yet has never seen that every A is C at all. If Jockey and Jenny never meet, the children they might have had, if they had married, have no legal rights, and so ends my *polemic*, as the other Sir W. H. calls it.’

From Sir W. R. Hamilton *to* A. De Morgan.

[Appended to a proof-slip of p. (32) of the Preface to *Lectures on Quaternions*].

‘Observatory, *January* 28, 1853.

‘If for any reason you dislike this mention of your name, it would not be too late to modify or suppress it. A recent re-examination of the *Annales* for 1813, by J. T. Graves, confirms fully my recollections respecting Argand. Graves gives me several references, which, as I have not lately verified them, I scarcely like to print, at least without acknowledgment.’

From the SAME *to the* SAME.

'OBSERVATORY, *January* 31, 1853.

'You and J. T. Graves, between you, will at least drive me into doing, what one would think that common sense might have taught me to do, namely, the *reading* before I write, or at least before I venture to *print*. Fortunately my sheet *d* is not yet printed *off*, and therefore, instead of *acknowledging* a mistake, if I have made one, it will not be too late to *correct* it. *You* suspect that I have attributed *too much* to Argand; *Graves* is positive that I have attributed *too little* to him; and begs me to re-examine the *Annales*, before I extenuate Argand's merit, by publishing the words "method of Argand *and Français*."

'As to Buée, he is again gone pretty much out of my head, although I read nearly through a carefully collated copy of his Paper not long ago. Are you sure that we do not read *him* by our own lights, and see in his Essay, more clearly than he saw himself, the rules of addition and subtraction of lines? My attention was directed rather to the question whether he possessed what we may for the moment call Warren's rule of multiplication, from which, in his No. 46, Buée has formally cut away all sort of claim from himself. Without re-examination I could not venture to assert; but to you I shall just throw it out as a hint for investigation, that Buée *perhaps* did not see the general rule for addition more clearly than Wallis. Observe that I refer to chapter LXVIII. of the latter (*London*, 1685, according to my memorandum of transcription; J. T. Graves has cited the book to me as *Oxford*, 1683; probably you can decide that question), and not to the preceding chapter, which Gompertz has referred to (in his second tract on Imaginaries, &c., London, 1818). Wallis seems to me to consider AB and Ba as roots of his quadratic, *because* their *sum* (or at least their *quasi*-sum) is the straight line $Aa = b$, and because their *semi-difference* (or *quasi* semi-difference) is $= BC =$ the imaginary radical realised, where C is the middle point of Aa. Buée's *seeming* "proportion," in his No. 35, is in fact (as he himself remarks) an *identity*. You are likely to know much better than I do; but I doubt whether Buée's Paper, from its vagueness, and what I own, between ourselves (for if I said it publicly, I should be greeted with the *Quis tulerit Gracchos*), appears to me its

intolerable licence of interpretation, may not have done more harm than good in England, to the cause of our friend $\sqrt{-1}$.

'I have recently been *reading* (and it is curious that sometimes, when otherwise in mental activity, I seem to myself unable to read a page, or almost a sentence of German) more than a hundred pages of Grassmann's *Ausdehnungslehre*, with great admiration and interest. Previously I had only the most slight and general knowledge of the book, and thought that it would require me to learn to *smoke* in order to read it. If I could hope to be put in rivalship with Des Cartes on the one hand, and with Grassmann on the other, my scientific ambition would be fulfilled! But it is curious to see how narrowly, yet how completely, Grassmann failed to hit off the Quaternions. He published in 1844, a little later than myself, but with the most obvious and perfect independence.'

From the SAME *to the* SAME.

'DUBLIN, UNIVERSITY PRESS OFFICE,
'*February* 2, 1853.

'I have just been "rung out" of the College Library here, which I do not find it often easy to visit. But though the time was up, I had been able to satisfy myself that my recollection, and that of J. T. Graves, about Argand, was right, as borne out by Gergonne's 4th tome of the *Annales* (1813 and 1814); and that my recent fault consisted chiefly, as Graves had thought, in putting Français on a sort of equality with him, as a discoverer. Argand was (in 1806) *the inventor* of the method of *multiplication* (combined with that of addition) of directed lines (lignes dirigées) *in one plane*. He *speculated* also on imaginary exponents, but I am not prepared to say more on that subject.

'I have struck out the word "Français," in line ten from the foot of my note in page (32), and substituted for it the confessedly vague word "others," which may, I think, pass *there*: and in line three from foot, to avoid repetition, and to mark my own opinion of Argand's* merit as an inventor, have suppressed the words

* 'Argand claimed, in 1813 or 1814, and had his claim allowed by Gergonne, the latter *seeing* his publication of 1806, to have in that publication (of 1806, which possibly our friend Peacock possesses) established as a FUNDAMENTAL PRINCIPLE (regarded by himself as properly a GENERALIZATION of the *rules of the* SIGNS) the *equality* of *angles* in a *proportion* between *directed lines* in one plane. I may be pardoned for attaching extreme value to this principle, since

"and others." Also I have added :—"A few additional remarks and references will be found in a subsequent note."

'I am not quite so enthusiastic to-day about Grassmann as I was when I last wrote. But I have read through nearly all of what I could procure of his writings, including a subsequent commentary (in German) by Möbius. Grassmann is a great and most German genius; his view of *space* is at least as new and comprehensive as mine of *time;* but he has not anticipated, nor attained the conception of, the *quaternions*, even so nearly as I guessed that he might have done, from a notion hastily taken up, of what might have been his meaning (and what it *was*, I *very* dimly know even *now*), in his doctrine of "eingewandte multiplikation." I quote from memory. His *outer* products (aüssere) I think that I *do* understand; and that is saying something for a person who has not learned to smoke. And even his *inner* products, published subsequently to the *outer* ones (in 1847), I can swallow pretty well. In fact, the "inner products" of Grassmann have much analogy to my "*scalar parts*" of a quaternion, and his "outer products" to my "*vector parts.*" If the notion of *combining* them had occurred to him, *he might* have been led to the quaternions; but those he seems to me to have altogether failed to perceive. Yet I think that my own researches, or speculations, would have a better chance of being *appreciated* in these countries, if readers had first been put through a sufficient course (or dose) of Grassmann. I must say that I should not fear the comparison. You tolerate egotism in correspondence, and I remain,' &c.

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*February* 5, 1853.

'Your two notes received, and attentively read. Your part, when you have duly advised with others, is to have an opinion and

it is the grand link between Double Algebra and the Quaternions. Addition of lines is so *very* easy, and has occurred to so *many* persons, that it is scarcely worth talking about; nor do I insist on Argand's having "first formally maintained" *that* doctrine also; but I wish that you, or Peacock, or both, would "formally examine" that question. How near did Wallis come? Did Buée go the whole way (about *addition*)? Had Argand been FORMALLY anticipated, even in *that* slight and easy matter, as it now appears to us?'

hold it; that is, as the lawyers say, when you come to a certain point, you find the *habendum et tenendum*. Don't, for your own peace sake, be a diffident man, and feel bothered, but take a side and care for nobody. But remember, as far as you like, Arago's apt remark in speaking of the invention of the steam-engine. He says a watchmaker would be struck all of a heap if he were asked, Who invented the watch?

'Argand I do not remember enough about; if he really envisaged the logarithmic property of the angles, he made a most decisive step. But you had best give your current opinion, as such, not committing yourself irretrievably, for priority matters are the things on which opinion changes wonderfully by mere digestion; and further, those who attend to quaternions want to have you out, not in the old sense, but in the peaceful way, with no knife but a paper knife.

'Your half-sheet reads well—I mean that it looks like deliberation and research. I got your note, putting it that perhaps I might dislike my name being mentioned in a particular way—I believe as the inventor of the happy phrase *double algebra*, and that you would not object to change it. What should I dislike in such a mention, or in any mention which arises in the current of free thought? Just look what your double (I speak as a nominalist, not as a realist) says of me in his philosophical *discussions*—at which I did not feel annoyed one bit—and then fancy me, on that *datum*, disliking anything said in a complimentary way because it was this way and not that way.

'You evidently live among susceptible people, and I live among no people; for, except to my pupils, I do not talk to a mathematician once in three months. So my skin has got so thick that neither can it be pierced nor tickled.

'Yesterday (February 4) was the thirtieth anniversary of my starting for Cambridge.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *February* 9, 1853.

'There seems to be but little chance of our getting up a quarrel between us. I won't be drawn into one on Theology, nor you on Mathematics. I suppose that I must try Logic, of which I once

was fond, and seek to maintain against you some position or theory of my namesake. From a book lent to me by the latter, when I was at Edinburgh in 1850, on a sort of indefinite tenure, I shall extract a few out of many lines by Leibnitz. Grassmann spells the name Leibniz: in Latin I find it printed as Leibnitius. Have you received or seen the Berlin medal of that great man?

'I have left so many remarks of yours unanswered, that it would be very unreasonable if I were to expect all my own sayings or writings to be noticed; and therefore cannot complain of your not noticing what I said about Grassman, but shall just observe, that if you have any curiosity to know anything of the result of my recent Nebuchadnezzarological reading (my daughter looking over my shoulder is amused at the folly of philosophers), it will be quite consistent with my humour to inform you. To the public I am likely to say but *little* at present about Grassmann; for I find that beyond the rule for *adding* lines, which he seems to have independently worked out, whereas I took it from Warren, we have scarcely a result in common, except one thing which *is* (in my view) important, namely, the interpretation of $B-A$, where A and B denote *points*, as the *directed line* AB. He comes to this, in his page 139 of the *Ausdehnungslehre*, after *long* preparations, and ostrich-stomach-needing iron previous doses. I, knowing nothing of this result, as in any way arrived at by him, STARTED with the same interpretation in my Lectures, in 1848, having printed the same conception some years earlier, and having been familiar with it (*see* Pure Time) for a *long* time before.

P.S.—My *old* interpretation of $B-A$ was, that it denoted a *time-step*, A and B denoting *moments;* but it scarcely requires my assertion that I saw, LONG AGO, that the same symbol might just as well denote on the same plan a *space-step*, A and B being points. With this view I was quite *familiar*, *before* the invention of the Quaternions. Argand did not anticipate YOU. He thought that $\sqrt{-1}^{\sqrt{-1}}$ was an imaginary (so to speak), or a transcendental of a *higher order* than $\sqrt{-1}$ itself.

Extracted (for Prof. De Morgan) from a Poem by "Godefridus Guilielmus Leibnitius."

"In Obitum Joannis Friderici, Brunsvicensium et Luneburgensium Ducis, ad Ferdinandum Episcopum Paderbornensem et Monasteriensem.—CIↃIↃCLXXX."

" Verum me mea sors et inexsaturata cupido
Detorsere alio, mentemque egere per omnem
Encyclium variis sinuatum anfractibus orbem,
Artium et omnigenum monstra, Algebramque rebellem
Vatibus, eque schola tenebrosi lumina Scoti,
Clamosumque forum, tectamque ambagibus aulam,
Desertosque voco nequicquam transfuga Musas. (lines 71 to 77.)

O mihi si color ille foret super usque juventæ,
Carmina cum pangens non inficianda Camenis
Tercentum versus fundebam luce sub una, (lines 21 to 23.)

Omnia tentabat discendi nobilis ardor. (line 182.)

Et quæcunque olim cupiit didicisse tuetur." (line 218.)
(Copied by W. R. H.)

From the SAME *to the* SAME.

'*Observatory, February* 11, 1853.

'With the help of a remembered hint from Cotes (*Harmonia Mensurarum*), and of Herschel's Section XI., on Circulating Equation, I hit off the following little theorem just now:—

'If
$$u_{x+2} = 2(2x+3)u_{x+1} + u_x,$$
$$v_{x+2} = 2(2x+3)v_{x+1} + v_x,$$
$$u_0 = 1,\ u_1 = 1,\ v_0 = 0,\ v_1 = 2;$$
then
$$v_\infty = (\varepsilon - 1)u_\infty.$$

'As an arithmetical verification:—

$u_2 = 6 \cdot 1 + 1 = 7$; $u_3 = 10 \cdot 7 + 1 = 71$;
$v_2 = 6 \cdot 2 + 0 = 12$; $v_3 = 10 \cdot 12 + 2 = 122$;
$u_4 = 14 \cdot 71 + 7 = 1001$; $u_5 = 18 \cdot 1001 + 71 = 18089$;
$v_4 = 14 \cdot 122 + 12 = 1720$; $v_5 = 18 \cdot 1720 + 122 = 31082$;

$$\frac{1720}{1001} = 1{\cdot}718\ 281\ 718\ 3;$$
$$\frac{31082}{18089} = 1{\cdot}718\ 281\ 828\ 7.$$

[In a subsequent note, written after he had consulted the volume, he states that "Cotes calculates as far as this, but by a longer and less simple arithmetic, involving intermediate fractions, and a periodically varying law."]

'The next approximation to $\epsilon - 1$ is

$$\frac{v_6}{u_6} = \frac{685524}{398959} = 1{\cdot}718\ 281\ 828\ 459.$$

'This approximation, as you see, is attained in *five* steps from the data.

[The note of Feb. 12 concludes as follows:—]

'The facility is striking with which this modification of Cotes's method gives so many decimal places for ϵ: but what is better worth noticing is the simplicity of form of the equation in differences, when freed of the periodical factor. I merely wished for a little exercise, just to keep my hand *in*, and am so far from being fresh in these matters, that I forget whether my first equation belongs to any known and integrable form. Accept this trifle, and believe me to be,' &c.

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*February* 12, 1853.

'I did not at first see the Nebu &c. logical pun, though I thought of Grass, and feeding with the beasts of the field.* I thought you alluded to having been turning yourself out among Argand, Buée, *et hoc genus omne*. I should like to hear about Grassmann—whom I am not likely to read. Between ourselves, I am disappointed with Germans—almost always. I have a new theory—take it. German intellect is an excellent thing, but when a German product is presented it must be analysed. Most probably it is a combination of intellect (I) and tobacco-smoke (T). Certainly I_3T_1 and I_2T_1 occur; but I_1T_3 is more common, and I_2T_{15} and I_1T_{20} occur. In many cases metaphysics occurs (M); and I hold that $I_aT_bM_c$ never occurs without $b + c > 2a$.

'N. B.—Be careful, in analysing the compounds of the three, not to confound T and M, which are strongly suspected to be isomorphic. Thus, $I_1T_3M_3$ may easily be confounded with I_1T_6. As far as I dare say anything, those who have placed Hegel, Fichte, &c., in the rank of extenders of Kant have imagined T and M to be identical.

* [De Morgan forgot that the joke on the name was his own. *Supra*, p. 425].

'I testify to your complete apprehension of $B-A$ as a step, and to the difficulty I had in making the step, when I first saw it. It is singular what difficulty a certain extension will give to persons used to extension. I once used $\times a$ and $\div a$ as separate symbols to D. F. Gregory, and a letter or two of discussion passed. It was not for weeks that he wrote me saying he saw what I meant; and yet $+a$ and $-a$ ought to have suggested the meaning immediately.

'There you are saying that I wanted to draw you into theological discussion—again—when I take the ghosts of Priestley and Horsley to witness that you fired shots, and I only fired in return. As to discussions on priority questions I am too wary. As I said before, it takes years of digestion to have an opinion. If you try logic, I shall say that I have already difficulty enough in persuading people that I have *not* been fighting you on that subject; and how many expressions of regret I have nipped in the bud about what a sad thing it is that mathematicians should fall out, and chide, and fight, I cannot tell you. But if you will take one of Sir W. H.'s notions to defend, take the following:—

'An affirmative proposition is merely an equation of quantities, of which coalescence of terms into one notion is a consequent.

'I should say *vice versa*. Every man *is* an animal; the two terms are *pro tanto* in one notion; *whence*, So many men so many animals. Sir W. H.'s language would justify us in declaring he makes "So many men so many animals" to give as a consequence that the men *are* animals. This is not what he means. I have found him out at last. *He cannot abstract quantity.* The fun which this assailant of mathematics is destined to make, when this becomes generally known, will be worth putting all into one book. Quantity in his mind is essentially attached to and inseparable from the thing quantified; and when he equates quantities he identifies the *res numeratæ*.

'Leibnitz's verses are interesting. The modern German writes Leibniz; so does Edlestone, in his publication of Newton's letters—and I think the publication of Leibnitz's letters in Germany.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 1, 1853.

'Are you gone to fairyland? If so, will writing "to be forwarded" on this be of any use? I have an indistinct remembrance that a long time ago you were engaged on a work on Quaternions. Did it ever appear?

'Seriously, have you been unwell, or how comes it that you are not out yet? I think it very possible you may have opened a new vein.

'I heard of you through J. T. Graves.

'N. B.—It was not the day that reminded me of you.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'EARTH, DUBLIN, *April* 4, 1853.

'You have been most correctly informed, or have made a very sagacious guess. I have just returned from fairyland: in the language of mortals, I have just finished a long (to me) charming, and (at all events) successful investigation, in the application of the bi-quaternions to the inscription of polygons in surfaces.

'But I must not quite forget that I have a book nearly ready for publication; and I have just been writing to an old friend, who had inquired respecting its progress and prospects, that I *wish*, and *almost hope*, to get it out *this* month, or at least before the 1st of May.

'For the moment I must merely sign myself, &c.

'P. S.—I write at the close of a Council Meeting of the R. I. Academy.

'Talking of *days*: this 4th of April would have been a birthday of a sister of mine, if she had lived, who *was* a genuine *poet*: *I* can only pretend to the poetical temperament and feeling. It has sometimes occurred to me, that Mrs. De Morgan might accept a copy of a volume of my sister's poetry, which was printed several years ago, but is probably very little known.

'I have been at a *few* Viceregal balls, and otherwise a *little* more *out* lately than I sometimes am.

'They continue to summon me on the Moore Committee, and I left awhile ago my card for Lord Charlemont (the son of the President of the Irish Volunteers), on my way to attend the Council here, of which I have been a member for (I suppose) a full quarter of a century.

'What *old* people these recollections make us! But we have here a frequent infusion of new blood, and, for instance, have lately elected Lord Talbot de Malahide, who deserves it on many accounts.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 14, 1853.

'Yours of 4th inst. wants a specific reply as to the volume of poems, and it is that we shall have great pleasure in receiving it. I don't know why you limit your own poetical qualifications to the feeling and temperament. If you had given your time to the practice of poetry you would have succeeded, and quaternions would have fallen to the lot of some other. I am not sure you are right in your qualification that the 4th *would have* been your sister's birthday if she had lived. The birthday being only the *anniversary* of birth, is it not the birthday for ever?* You know I am keen about all almanac matters. I have been in metaphysics lately, and writing, among other things, something in answer to your namesake, which will appear in time.

'I am now clear that all opposite terms in logic come under *plus* and *minus;* that is, that there is a *permanent* distribution of them which makes all truths contain either *none* or an *even number* of the negatives, thus carrying the rule of signs into all our thinking processes.

'You will smile when I tell you that *universal* is certainly −, and *particular* is +; that I suspect *subjective* is −, and *objective* +, but am not sure. But I am only at the beginning. The idea of class conveyed *to* the mind by the *convenientia singularium* has one sign; the *attribute* scattered *from* the mind over the individuals has the other. Disjunction, as in *either A or B*, is +; conjunction, as in *both A and B*, is −.

* [Hamilton writes in the margin, "Yes, I grant it."]

'All this means that I have many cases in which, when the above terms enter in opposition, the placing against each term the sign which it bears *in a fixed table* will always give an even number of negative signs. In this I am working my own way inductively, without a failure as yet. I have never come to a case in which I am obliged to say—the term A is certainly + in this proposition and − in that.

'Don't wait to publish until you have exhausted quaternions; "rusticus expectat," &c; and mind you give your mind a bit of fallow when it is over; there ought to be a rotation of crops, and an interval between each. Now you can easily manage a four-course succession—Mathematics, Poetry, Metaphysics, and the Lord Lieutenant's balls.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 5, 1853.

'There has been much sickness in this house for some time past, but I hope to write to you soon. The illness of *all* my children has prevented me from attending to other things, and especially from getting and forwarding the volume of poems for Mrs. De Morgan.'

'*May* 7. . . . My children are somewhat better.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*May* 9, 1853.

'I am sorry to hear of your troubles of sickness, but I infer from your words that there is no matter of serious uneasiness in it. Two half sheets received, and I am glad to hear that you are really nearly at the goal. When you reflect that you thought of being out last Christmas twelve-month, you will see that you had a stiff job, if you do not feel it.

'I have been hearing the spirits, or whatever they are, rap upon the table, and answer questions put *mentally*, while the American lady held both her hands extended in the air, to make sure of her. This is more puzzling than $\sqrt{-1}$. When you really hear something staggering from an eye-witness, ask me for details

to knock you down quite. Our *thoughts are read*, somehow, you may swear.

'I have nothing to say mathematical; my thoughts have all been on logic.

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 21, 1853.

'I have just received the three sheets, including the one marked "the end." You seem fairly landed at last. Moreover, I saw only yesterday, my wife having had it some days, the volume of poems for which I am to give her very best thanks, and my own. Poetry runs in families more than mathematics, though who can say what women might do if they were made to look steadily at such pursuits?. The only person to whom I ever taught the Differential Calculus, who dashed aside the common difficulties, and settled at once on the points of principle which the investigators have fought and differed about, was a woman—Lady Lovelace. Our colloquies were in written correspondence, and one of these days I shall collect some of her assaults upon the first principles, made before she had had practice enough to differentiate with common correctness. They will be very curious. Her father would have sworn at her, if he could have known that she had a mathematical head.

'I have not read the poetry yet, but shall begin in a day or two.

'Talking of poetry, see what I have met with in Sylvanus Morgan's *Horologiographia*, 1652:—

'If Tellus winged bee,
The Earth a motion round,
Then much deceiv'd are they
That it before ne're found.

'Solomon was the wisest:
His wit ne're this attained,
Cease then, Copernicus,
Thy Hypothesis vain.

'This runs smooth. The following verses, in Hylles' *Arithmetic*, 1600, show that smooth versification was not impossible to a

mathematician. I put them on a separate paper as the measure is long :—

"But late I sawe a fountaine faire, most stately to beholde,
At top whereof a Lyon stood, made all of molten golde,
Out of whose mouth, his eyes and foot, the christall liquor gusht,
And in a bowle of Iuorye, with silver streames it rusht.
This fountaine was so finely fram'd with vices* for delight,
That lions mouth and eies might close, and stop their courses quite.

* [*vices*, pro hac vice, mean *changes*].—Transcriber.

"Now if his issues all were stopt, save onely his right eye,
In 2. dayes it would fill the bowle, though it before were drye.
And if the left eye onely run, three days then serues the turne,
His foote alone 4. dayes must have, the same thing to performe.
But if his mouth alone do spoute, but onely 6. houres space,
The water will just fill the Bowle, and neuer misse an ace.

"Now if these issues all at once, should keep their course at wil,
Then show me in how short a time this empty bowle should fil."

'"This prety conceite hath been gathered from Master Hylles his Arte of Vulgar Arithmeticke imprinted at Londone in 1600 by Gabriel Simson. The selfe same question was dulye woorked by Scheubelius, as also by Peucerus, who discorsed of the same in Latine: but Master Hylles rendered the premises into our English tonge, for the better contentment of those who mighte not rede except therein. And truely meseemeth that his worthy paines may goe to showe forth the manner wherebye our English, which aforetime was pore and vile, became musical to heare, although it were a numerist who should speake, so it were one who made diligent studye in Shakspeare and Spencer their poemes."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory of T. C. D., *June* 30, 1853.

'Now that my book is actually printed off, or at least the last pages signed for press by me, I feel almost as if I had ceased to be connected with it. Perhaps it is a common thing; yet it seems to me curious to observe it in my own case, how, after being days haunted by the "malice of one luckless word," (Wordsworth) —for instance, I sent a messenger to Dublin yesterday, on purpose to ascertain that the word "now" had been omitted in the clause, or part of a parenthesis, "the symbols $\times_r$ being in fact what

M. Cauchy *now* calls keys"—I say, how, after being thus anxious about a mere *word*, so long as my anxiety could be productive of any practical effect, I completely dismiss all such anxiety, when any writing of mine has become *publici juris;* or even when I have signed a sheet for press, and know that it is actually printed off.

'It is so strange a feeling to me this morning to have no longer any responsibility as to composition or correction of my book, that I am horribly at a loss what to do, and suppose I must take up astronomy.

'Are you aware that by a strange mistake, which I believe that I must take the trouble of in some way publicly correcting, the Royal Commissioners in their Report on the University of Dublin have precisely *quinquisected* the diligence, such as it has been, and I do not say that it has been great, of observation here? I caused the *lines* of the *sheets* of observations to be counted, and partly counted them myself, for a certain period of twenty-four years; they amounted to 44,430 (*Blue Book*, Evidence, p. 102), and were counted precisely on the plan adopted in several other Observatories, in forming "numbers for reference"; but the Commissioners have supposed the "lines" to mean *spider-lines*, or wires, and on an estimate of *five* to each single observation, have divided our poor forty-four-thousand, not by twenty-four but by *one hundred and twenty*, in order to strike the annual average. Codrus had nothing, but poor Codrus lost that nothing. How many people's diligence would bear to be divided by five?

'P.S.—The vast majority of those forty and odd thousand observations has been made by my assistant, Mr. Charles Thompson, retained from Dr. Brinkley's time. His diligence and accuracy appear to me to be increasing rather than diminishing. I must add that I have spent with him a great many nights of observing in the dome which are not included in the foregoing account.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*July* 2, 1853.

'Your proofs and fair sheets duly received, and I really congratulate you on your having done with it; and I shall be glad to see it all stitched together. I think I must look for a new double

algebra, as follows:—Sir W. H. of Edinburgh is always afraid of my robbing him. Sir W. H. of Dublin is always afraid of robbing me, as per letters to me appears. Here are interpretations of + 1 and − 1. I think the Edinburgh man the most *positive*. Now there is a third Sir W. H. in the Directory. I intend to get acquainted with him to see if I can get any interpretation of $\sqrt{-1}$.

'I shall look into Argand's whole case as soon as I can get his book, &c. If you are right, Double Algebra should be called Argand's lamp.

'I did not know anything of the Report about T. C. D., and so knew nothing of the reckoning of the observations.

'As in matter, so in mind. You cannot raise crops of one character for ever. You must either let your mind lie fallow or take to something else. It is astonishing how new ideas will spring up, if you let the machinery have rest.

'I think you should take up observation for six weeks, sonnets for six weeks, and extract the square root of some wholesome integer to fifty decimal places, just to feel your way down to earth again. Mere calculation is very good after dissipation.

'I return Graves's letter.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 7, 1853.

'My book will not be *quite* off my mind till it is actually published, and on some few counters of booksellers—to be at least looked at there, whatever may be done about buying it—and also till the chief presentations shall have been decided on and made.

'But I will tell you of an old speculation of mine, of a quite different kind, which came with some force into my head this morning, and may possibly set me soon on some such arithmetical computation as you rightly judge to be at times a relief, after more abstract reasonings. Many years ago (in 1839, and even earlier,) I made some investigations respecting what I supposed to be a new symbol of operation, and am still unable to refer to any other person. This was the characteristic I_x, used as a symbol of *definite* integration from 0; so that $I_x f_x = \int_0^x dx f_x$.

'. . . You see that I_x is not exactly the same thing as D_x^{-1}. . . . I once made the Argand joke to Herschel, who seemed to me to attribute too much to Buée.'

From A. De Morgan *to* Sir W. R. Hamilton.

'*July* 11, 1853.

'I don't think the subject you have hit upon for your recreation quite the thing after quaternions. It is *toujours perdrix.* It reminds me of the sailor's three wishes. "What first, my man?" "All the rum in the world." "What next?" "All the tobacco in the world." "Well now, the third?" "Let me see—well, I don't know—I think I'll have a *little more rum.*" I proposed to you something without any exercise of mind—such as calculating the square root of 4 to fifty decimal places $4{\cdot}00\ 00\dot{0}$ (2·000, &c.)—It is true that $\int_0^x dx$ is not $\left(\frac{d}{dx}\right)^{-1}$, Q. E. D., meaning *quod est dolendum.* The properties of Δ^{-1} and d^{-1} are worth studying. I suspect that $\left(\frac{d}{dx}\right)^{-1}$ is $\int_{-\infty}^{x} dx$. . . . There are plenty of difficulties . . . But you ought not to look at them these three months.

'We are all here in expectation of a decimal coinage. I am busy at the commonest things relating to the theory of differential equations—in which there is no sufficient accuracy in anything simple—and less in the complicated.'

[In the foregoing two letters I have omitted the mathematical work of both correspondents, feeling that to print it in *extenso* would overweight a volume intended for general as well as scientific readers, and contenting myself therefore with indicating for the benefit of the latter the subjects discussed—a course I shall have to follow in the remainder of the correspondence. But as the original letters will be deposited in the Manuscript-room of Trinity College, it will be possible for the scientific student to refer to them for the omitted portions.]

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 16, 1853.

'My book is actually out. I got a person to *buy* a copy for me yesterday evening, a frolic which will cost me 5*s*.

'Is Herschel still at 32, Harley-street? The *Athenæum* lately

printed Henley-street. Please answer *this* question as soon as you can, and excuse the haste in which this note is written.

'Accept a copy of a newspaper.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*July* 18, 1853.

'Herschel is still in Harley-street. The *Athenæum* address is a misprint. I am glad you have actually bought your book. You may now add to your name the letters I. J. K., and some day perhaps your speculations will be L. M. N.-tary, but not just yet. You must not think that your I. J. K.-eries are only just above the L. M. N.-tary, because it is so in the alphabet.

'Your newspaper received. But what is in it? I don't know yet, as I answer yours at once according to your desire, proving that I can more easily make a pun than read a newspaper.'

[Pursuing the course above indicated, I here record that in his next letter (July 25) Hamilton asks De Morgan if he could detect, or refer him to some work in which he would find exposed, a fallacy into which he suspects himself to have fallen in deducing the method of least squares from the ordinary theory of probabilities, since his result differs from the translated paper of Encke on the subject. On July 29 he writes, objecting as well to Encke's as to his own previous result, and modifies the latter. This was not sent until August 1, with a postscript acknowledging De Morgan's answer, but a condensed statement of its contents was forwarded by Hamilton on the 30th of July. De Morgan (July 29) disputes "both you and Encke," and points out his reasons.* Hamilton's of August 1 confesses agreement with De Morgan in regard to the fallacies which misled both himself and Encke.

On August 18 Hamilton suggests a slight abridgment of

* As postscript to this letter De Morgan records characteristically an event of the day in the social life of the metropolis:—"The triangle *CAB* (see Euclid *passim*) is now the only CAB to be had in London."

De Morgan's argument in p. 67 of his *Differential and Integral Calculus*, where he derives the simplest case of the theorem of the mean value. He also points out an error in Moigno's *Leçons de Calcul Différentiel.* De Morgan on the 19th admits that the proof is subject to difficulties. "But a young Cambridge man, Homersham Cox, threw the material of Cauchy's proof into such an easy and beautiful form, that I have thrown away everything else in teaching." He then gives the proof of Taylor's theorem, now current in all text-books. He remarks incidentally:—"The principle of fluxions is one that always gives light whenever it can be used"; and adds at the end, "I wish my *Diff. Calc.* had never been stereotyped. I could make it a hundred times better now—as I think."]

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*August* 22, 1853.

'I once solved the following problem:—

"A set of books is found to have n volumes, which range from 1 to n. What is the probability that it is complete? Of course it is very unlikely, if n be considerable, that the losses shall have been after n, and all after n."

'There is a kindred problem:—"Supposing that from m to $m+n$ is complete, n being considerable, but the set from 1 to m incomplete: what is the probability that the set was not originally complete?"

'Now for the application.

'All my fair sheets of the Quaternions have been together since I first received any, and on making them up they appear thus: The *body* of the work *complete. Table of Contents consecutive* to p. lxxii. *Preface* incomplete. I *have* three title leaves and dedication, *d*, *f*, *g*, *h*, and no others.

'I think the rest can never have been sent.

'I have been laid up a day for eating something or other, I suppose, but it was worth the annoyance to get a cool view of the phenomena of a feverish night. Everybody knows what an

excessively disagreeable thing this is, but it generally defies description. Now, being only on the verge of this infliction, and not deep enough in it to feel utterly lost—in purgatory, as it were, and not in hopeless perdition—I should, from my own very distinct recollection, give the following account of the whole thing:—

'"The individual is asleep, but does not know it. He fancies himself awake, seeking sleep, which is not a quiescent state, but the result of some exertion in his own line. This exertion is impeded by some incongruity which he cannot get over. After great botheration, unconsciousness comes on, and the same thing is repeated again and again: while this repetition gives the idea of existence being only a kind of pattern like that of the paper on the wall—a repetition of one figure, with a dull space between.

'"In my own case, getting to sleep depended on the solution of a quadratic, of which I saw the roots before me, but could not get them out of the formula, and no wonder. They were always squares side by side, time after time."

N. B.—The squares were quite equal, and it was a pretty illustration of equal roots verging on the impossible.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,

'*August* 23, 1853.

'The theory of probability turns out false as to three of the sheets, which I therefore return. The mode in which these hid themselves—long to tell—confirms the theory, however, as to the fourth.'

[On August 22, Hamilton sends De Morgan a Paper containing a couple of "elementary illustrations of the reasoning" in "Inverse Probabilities" (De Morgan, p. 60), which he had written out for his son, and of which he says, "You are welcome to light your pipe with it." A short note of August 24 shows that he was still occupied with De Morgan's "little work" in the *Cabinet Cyclopædia*, on Probabilities. In a postscript he records an observation of the comet which he had seen the previous night.]

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*August* 26, 1853.

'I cannot answer your comet question, but I have begged Hind to drop you a line as soon as he finds any reason to think it is the comet which, as I phrased it, he took lodgings for a year or two ago.

'Last Monday morning I got a letter from my wife (at Herne Bay) written on Sunday evening. On the outside was written—"A comet above the moon—with a tail—the tail scintillates." This was pretty well for the naked eye. Her mother saw the comet of 1811 before it was known to astronomers, and pointed it out to Professor Vince, who was lodging near them, at Ramsgate. He laughed exceedingly at the idea of it being a comet, but her eye turned out right. She also once saw the old and new moon in the same twenty-four hours.

'Your Paper on Probabilities I shall not light my pipe with. There is always something new in a new person's illustration.

'I see you did not notice, in the part you have read, a thundering error, on the simplest point possible. Nor has any one else noticed it, except myself, who have printed a sarcasm against "a recent writer" that I may be able to prove I found it out before anybody. It is not in the enunciation of a principle, but in an application. Everybody makes errors in probabilities, at times, and big ones. Laplace and Poisson have both fallen into the assumption that if $\phi(x, y) = 0$ be the best (most probable) equation to find y from when x is given, it is therefore the best to find x from when y is given, which is such a mistake as a little girl once made to me: I called her a daughter of Eve, and she retorted, "Then you're a daughter of Adam."'

From Sir W. R. Hamilton *to* Professor De Morgan.

'Observatory, *September* 7, 1852.

'I have not had time to hunt for your "thundering error," but as a blindfolded child asks is he *hot*, meaning *near*, I shall just inquire whether it was in the *first* chapter of your *Cab. Cyc.* Essay, which I read with less care than the two following. I have read a

good deal, up and down, of the book, but shall scarcely attempt to go farther *systematically* at present, unless I can find my lost sheet of Laplace's great treatise, or get hold of your essay in the *Encyclopædia Metropolitana*.

'I perfectly understood, long ago, though it was somewhat *stiff*, Laplace's investigation of the theorem, that

$$1 \cdot 2 \cdot 3 \ldots n = \sqrt{2n\pi} \cdot \left(\frac{n}{e}\right)^n \left(1 + \frac{1}{12n} + \&c.\right).$$

The third term of the series was something like $\frac{1}{288n^2}$ as I *think*, but I have no book on the subject at hand, and certainly shall not attempt *just now* to re-investigate the expansion for myself. Had Euler anticipated Laplace in the *result*? for I suppose that Laplace's *method* was his own.

'The Royal visit of last week occupied nearly all the attention and interest of us Eblanians; but I was glad to receive, from a postman who was walking out, a letter from you about ladies and comets, &c., as I was on my way to join the Provost and others of T.C.D., in my Doctor's robe, on the Monday morning of the Queen's entry, of which we saw very little this time. Four years ago, the College erected a large platform, when we could all *see*, and *be seen*, comfortably; but our fine gowns, and those of any lady-acquaintances of ours, were quite thrown away in 1853! I fared not much better on the Tuesday morning, as a season ticket-holder in the Exhibition, but had the consolation to think that I had lost my two or three good places, at various stages of the Queen's progress, by yielding them to ladies; and when her Majesty was gone, I spent (what I had not so completely done before) an entire unbroken day in the beautiful and curious building. On Wednesday morning I received a card of invitation for Lady Hamilton and myself, to meet the Queen and Prince at the Viceregal Lodge on the evening of that day, and I persuaded my wife to come, though she is very shy about going out. There seemed to be no actual presentations, but it was a pleasure to *see* the Queen by candle light, to hear the music which was performed, and to meet acquaintances. Lord St. Germains, when Her Majesty had retired, came up to me, and gave me to understand that the Prince wished to receive *personally* a copy of my book, which I had proposed

to *send* him through his secretary; and on the Saturday, about 3 o'clock, I had a pleasant interview (which was *strictly* a *tête-à-tête* one) with His Royal Highness for that purpose. There's *gossip* for you and Mrs. De Morgan!

'P. S.—I had the honour of *meeting* the Queen and Prince at the Viceregal Lodge in 1849—I do not mean of *conversing* with them: and she was pleased, soon afterwards, to command Colonel Phipps to write to me, and to thank me for two sonnets which Lady Clarendon had the goodness to forward. But I am sure that I must have told you all this before.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*September* 12, 1853.

'You will receive by post a copy of my quarto tract on Probabilities, under circumstances which the title-page will explain, binding you either to survive me, or to sell your books in Dublin.

'My great error is in page 28 of the little treatise. Reaching this down to refer to, I find that I interpreted the vignette in the title-page as follows:—"The lady with the smallpox is owner of the ship which is going to strike on the rock. She says, 'If I die I'm in luck, and as for the sailors they may go to ———; life and ship are both insured.'"

'Stirling, if I remember right, preceded Laplace in the formula, so far as

$$\sqrt{2\pi n}\,\varepsilon^{-n}n^n.$$

The following:—

$$\sqrt{2\pi n}\,n^n\,\varepsilon^{-n+\frac{1}{12n}}$$

is right to terms involving $\frac{1}{n^2}$ inclusive.

'I deduce the first as done in my Trigonometry from

$$\frac{2.4.6\ldots 2n}{1.3.5\ldots 2n-1} = \sqrt{n\pi}$$

nearly, when n is very great. This is Wallis's. Laplace's *method* was *quite* his own.

'The gossip about the Queen is all new. I never saw a king or a queen in my life, except Louis Philippe just after his accession. I never saw the Duke of Wellington but once, for a few minutes, in the House of Lords—the only time I was ever there in my life. I never was in the House of Commons, or in the Tower, or in Westminster Abbey. I spent only one and three-quarter hours in the Great Exhibition. I never attended a meeting of the Royal Society or British Association. I never got further north than Cambridge, and never while at Cambridge penetrated to the northern extremity of the town. So much for me as a sight-seer and traveller. And yet I have been in three-quarters of the globe —*in arms*—not as a combatant but as an infant.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Carlingford, *September* 28, 1853.

'I fired off so many letters at you some time ago that you may have been surprised at receiving no acknowledgment of your note of the 12th of this month, accompanying the valuable present of a copy of your quarto essay on Probabilities. The fact is, that I left my Observatory on the 10th, proceeded, through Dublin, Kingstown, and Holyhead, to Chester; went thence on the 11th to Manchester, and on the 12th through Selby (where I enjoyed a Russian bath, hot vapour succeeded instantly by a cold shower) to Hull. At which place it was that, while sitting after dinner with a pleasant party, on (I believe) the 16th of September, your packet was handed to me, and was (by my host's permission) at once opened. Some people were there who knew you, and I amused them by a recital of some of the things and people which you had *not* seen.

'I took very little part in this last meeting of the British Association, having in fact arrived too late to take much, for the wedding of a near connexion of my wife's to a rich and young English lady took place in Dublin about the end of the week in which the Meeting opened at Hull, and prevented me from going early. But on the Thursday of the subsequent week (Sept. 15th), I joined the excursion party to Beverley and Flamborough, which was a very pleasant one. The day was delightful, and both the Beverley Minster and the Flamborough Cliffs were seen to great

advantage. In my visit to the former, I had the advantage of the company of Dr. Lee, of Philadelphia, who has seen all the great cathedrals of Europe; and on the latter (the cliffs) I heard Phillips and Sedgwick alternately lecturing, every now and then, as we reached one or another point of interest. Sedgwick was grand in his denunciation of the "base, bloody, and brutal" practice of shooting at seagulls in their breeding time; but admitted that he had done it himself long ago, "before he was civilised"! There was a bye-play going forward, which however did not succeed, to get, by some "correlation of physical forces," into the orator's hand a short gun which Mr. Grove was carrying.

'On the Saturday evening (Sept. 17th), I went from Hull to York, where I remained till the Tuesday morning, attending at the Minster twice on Sunday, and seeing the Walls, &c., with Phillips on the Monday, besides meeting Dr. Daubeny, of Oxford, and Mr. Babington, of Cambridge, and seeing the moon through an excellent telescope of Phillips's, and photographs of it made by the same. On Tuesday evening I visited Bolton Abbey, and again on Wednesday morning (Sept. 21st); went thence by Skipton to Morecambe, and crossed by the *Faugh-a-Ballagh* (for 5*s.*!) to Belfast, where I breakfasted and dined with my hostess of last year, and on the Thursday evening reached my old friend Thomas Disney here (at Carlingford), in time to anticipate some frightful equinoctial gales. Going out to see the quarries which are said to be curious, I remain,' &c.

[It has been stated in Vol. II. p. 692 of this work, that Sir W. R. Hamilton was asked to become a Trustee of the *Guild of Literature and Art*, founded by the first Lord Lytton, then Sir Edward Lytton Bulwer. The application was made through Professor De Morgan, and many letters passed on the subject, for the final reply of Hamilton was delayed till nearly a month had elapsed from the date of the proposal. The impatience of De Morgan at his friend's delay was amusingly manifested in the interval, but it will suffice to print here the commencement of De Morgan's first communication of the proposal, showing how it originated, some characteristic bits from succeeding letters, and Hamilton's considerate and judicious acknowledgment of the

compliment paid to him. He ultimately succeeded in obtaining his wish to be a simple Member of the Council rather than a Trustee.]

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*October* 4, 1853.

'Last night I attended a meeting of the constituent committee of the Guild of Literature and Art—a new formation for the benefit of authors—Literature including Science—C. Dickens in the chair. It is well taken up by first-rate literary men. The subject of *trustees* being brought forward, and a beating-up for good names going forward, it was suggested that we should look to Ireland among other places, and Dickens immediately suggested you—not I, mind—I said never a word. All I did was, when this meeting of literaries and arts—not a scientific among them but myself—had caught up your name in a manner which would have shown you that even portrait painters have heard of quaternions—all I did, I say, was to glorify myself by declaring myself a correspondent of yours, and to offer to ask you . . . I hope you will let me have a line to say you will accept.

'*October* 12. . . . I am just packing up the MSS. of Baily's account of Flamsteed, for deposit at Greenwich, with other papers. You and I are no hand-writers compared with him. The copies of the letters in the Appendix to Flamsteed are not perfect, many being printed from originals, so I do not collect them. The enclosed is a genuine bit of the manuscript, which will illustrate what I say, and you may paste it into p. 108, of your Flamsteed *in memoriam*.

'*October* 19. . . . I saw Lord Brougham with the Quaternions in his hand, packing up for France. I told him he had a *nut* in his hand. His teeth are pretty strong ones.

October 29. . . . I hope you have not been ill: if you have, it is the reaction after quaternions, and you must amuse yourself diligently. Meditate conundrums. For example: When there are three young ladies in a league, what proverb applies? Answer: A miss is as good as a mile. I consider this a mathematical

question: What English surname read forwards directs you to continue at dinner, and read backwards, *in Irish*, gives a reason for it? Answer: Eaton. Make a worse than this and you shall have the time for answering enlarged 24 hours. You have no idea what a comfort this straw paper is.'

[He had previously written 'This paper is made of *straw*, and if you write on it you will find your pen runs along it without catching.']

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 31, 1853.

'I have not been ill, but the illness of a friend very long beloved by me, combined no doubt with something of that reaction from hard study which you speak of, has so much affected my spirits during the whole of this now expiring month, that I have scarcely been able to attend to anything like business, and have left *several* letters unanswered, including even some from ladies—one of whom, with the professed object of inducing me to repay her in kind, has sent me a manuscript poem of her own, with some additional verses to myself!—nor have I felt up to making even one visit this month to the now closing Exhibition in Dublin, to which I had gone innumerable times, before my late excursion to Yorkshire and the North of Ireland.

'Still I did write, without sending, two or three notes to you, on which I cannot at this moment lay my hand, relating to the very gratifying honour which the members of the constituent committee of the new Guild of Literature and Art have paid to me by inviting me, through you, to become a trustee to one of their funds.

'If I had been a member of that committee, I should most *sincerely* (and not at all in the spirit of a young lady's No, nor of the traditionally believed-in saying of the *Nolo episcopari*) have made a very decided remonstrance against their adopting such a course; although the interest which I really feel in the new Institution, and the sense entertained by me of the compliment offered to myself, have rendered it difficult and painful for me to return a peremptory refusal.

'Scientific standing and social probity may be assumed; but they seem to me to be quite inadequate grounds for putting forward my name before the world, and especially before the world of London, in so responsible and prominent a character in connexion with this new Guild.

'If a trustee is to be, even slightly, of use as such to the Institution, he ought, I think, to possess wealth, *or* rank, *or* business habits, in a degree far greater than mine; and indeed it appears to me that a residence in or near London ought to be counted an almost necessary qualification.

'As to rank, my little "Sirship" (I am not a baronet) can go for very little indeed. As to wealth, though not an embarrassed man, I am anything rather than a rich one; and just now am looking forward, with great disrelish, to the prospect of being called upon to discharge, next Christmas, the large balance of my printer's bill. And as to business habits, although my recent procrastination about writing has been perhaps abnormal, and above the average, at least in corresponding with you, it may still serve to suggest the inconvenience of having to transact affairs by post with a person who seldom goes to London.

'For my own part, Irishman as I am, and desirous that Ireland should be in some degree represented in this beneficent and important undertaking, I must confess that I do not see the necessity of having an Irish *trustee* to any of the funds of the Institution. If I did so, I should risk the being supposed intrusive, both to them and to him, by suggesting to you that my old friend and brother-author, the Rev. Humphrey Lloyd, D.D., Senior Fellow of Trinity College, Dublin—who knows as much (perhaps) about literature, and more about art than myself—and who is in a very much more comfortable position as to fortune, would be a person very proper to be named. Or my *quondam* pupil, the Earl of Dunraven, would, by tastes and position, be well fitted for the post. But you will conceive that I have not thought myself at liberty to mention the matter to either of them.

'If I might *choose* my own place, it would be that of a member of the *Council*. Joined thus with many others, I could not seem to lookers-on to have obtruded myself; and yet it would be open to me to offer, from time to time, without an air of intrusion, any such suggestion or information as might appear to me likely to be useful.

'I assure you that it is my *real wish* to occupy some such less prominent (though still honourable) post. But if the Council or constituent Committee should desire to overrule these objections, you will not be surprised that, however satisfied *I* personally am of (not merely the integrity but also) the PRUDENCE of the conductors of the plan, still, in justice to my wife and others, I should expect to be allowed to consult a confidential law adviser, on the subject of the *security* afforded by the trust deed, or act of incorporation, against personal and pecuniary liability.

'You say nothing about the subscription expected. I should not like at present to *promise* more than an annual pound or guinea, though I might probably manage (councillor or not) to contribute a few more as a donation.

'Whether you *show* this letter or not—and I have no objection to your doing so—I beg that you will at least present to the members of the constituent Committee the assurance of my very high respect, and also of my sympathy with their design, and admiration of the personal sacrifices by which it has been signalised.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*December* 10, 1853.

'If you can say that Double Algebra has an inventor, you may say that a chronometer—as distinguished not from a watch, but from a turnip—has an inventor. I hold it to be a piecemeal job, and if my recollections are good, I could set Servois about as high as Argand. I hold with Peacock that Buée is the first formal maintainer of the exposition of $\sqrt{-1}$. See *Report on Analysis.* I do not hold Buée to have extended the whole system. There is no subject more likely to produce in time a priority controversy about *who* among those who forget first to settle *what.* The essentials of a subject are *subjective* things, so that different persons are really and truly the inventors to different people.

'*AB* being your good wishes about the new year, let $BA = AB$.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 15, 1853.

'To decypher the enclosed paper (No. 1) would, I am sure, be mere child's play to you, but *as such* I spent a part of yesterday evening upon it, and wrote (No. 2) in reply, to show that I had found out the key and could use it. I wished merely to divert my mind in every possible way, though I have grown interested again in quaternions and such things, and am also trying a few telegraphic experiments with my second son (Archibald), he being quite my teacher in them, and having stretched a wire of about 100 yards across my lawn, equal (he estimates) to four or five miles of wire of ordinary thickness, with two plates sunk in the earth, and a small water battery, by which apparatus he deflects very nicely a galvanometer needle in my study. All this is a preparation for our trying, on Airy's invitation, to connect galvanically this Observatory, after some time, with that of Greenwich. He (my son) has succeeded in connecting our transit and dome clocks, and is just going to show me the experiment. Tho', by the way, some sacrilegious person has lately stolen a brass tube, which had been his positive pole! and was left outside the house. The rest of the battery was, curiously, untouched. I am ashamed to say that I have not yet answered Airy's letter.

'But talking of cyphers, I enclose you another (No. 3), which also, though *far* from being a practised hand, or anything *like* it, I amused myself lately by translating. Even *this easiest form* of cypher, with *words* and with *one* symbol for each English letter distinctly separated (and I should not like to attempt any other, but think that I could decypher any passage *so* written, if it were *not shorter* than these specimens), might give instructive work to some more skilful person, if applied to any *foreign language*. So if you ever, as a Christmas amusement, send *me* any such, please to let it be in English, and on the above-said easiest plan. But a form of secret writing has occurred to me, which I can scarcely imagine how any human ingenuity could, without a hint, make out, although it has an absolutely *definite key*. If you know any one who likes trying such things, I would enclose a passage, or several passages, of English written on this new plan; and though I think a large sum might safely be offered as a reward, the

successful discoverer of the secret must be content with the tribute of my unbounded admiration! I could send you, if you choose, a sealed-up paper containing the key, and a translation, to be opened by you at some stated time. Observe that I do not bind myself to attempt the solution of any such problem in return.

'Now for the clocks!

'P. S.—I have a scruple of conscience about wasting the time of my old acquaintance, Dr. Hincks. by sending him anything in *my* cypher (cipher?)—he might, I am so audacious as to imagine, *give it up* at last, but not till after long efforts! *There's* impudence for you! Perhaps you have heard that, long ago, he obtained a prize of Fifty or a Hundred Pounds, which some one (I think it was Chenevix) had offered, in the confidence that it could never be claimed.

'P. S. No. 2.—You may *show* (or send) this note to Airy if you choose, as a basis for the "re-opening of negotiations between the high contracting powers."'

From the SAME *to the* SAME.

'OBSERVATORY, *December* 17, 1853.

'Your note of the 15th ought perhaps to have reached me yesterday instead of this morning; and, if so, I should sooner have expressed my sympathy in your parental anxiety for your daughter, at an interesting time of her life, and important crisis of her health. You know how apt I am to talk, at least to *you* (for I am somewhat more guarded with people in general), about my *own* daughter. A few years ago she was attacked by scarlatina, and though I was told that the sickness was infectious, I could not be kept out of her sick-room. Notwithstanding the celebrated song in the Beggars' Opera—I suppose sung by Macheath? or by the gaoler? (I must some day refresh my memory)—"I wonder any man alive Would ever rear a daughter," I would have always given the order "Tollito": and now that I have fallen on other times, I am apt to estimate the happiness of any friend of mine by the circumstance of his or her having a daughter. Of course the most exquisite *un*happiness might be derived from such a source; but that alternative we may dismiss from thought. I have been

showing *my* child, Helen (what is *your* daughter's name?), at her own wish, ever so many astronomical things to-day—not that she understands them all.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*January* 10, 1854.

'I think, if I remember right, that I wrote you a short note, telling you how completely the illness of my eldest daughter prevented my even reading your notes. On the 23rd of last month she died in her sixteenth year... I am just beginning to recover—I will not say my spirits—but power enough to attend to things not absolutely essential. I shall take your different notes soon. I read them hurriedly as received, but have quite forgotten the contents. In fact, all my papers look as they might if I had been suddenly carried off, all round the world, and set down again at my desk.

'I see, however, that I have a sheet to return [questions set by Hamilton at the Examination for Bishop Law's prize] which I hope you have not felt the want of. Do you mean to make Dublin the first school of this kind of analysis in the world? If not, what do you mean?'

'*February* 13.—I have written no letters at all beyond obligatory ones—until now—but I am very well, with many thanks for your kind inquiries. My wife was very much obliged by the verses, which she liked very much. Of mathematics I have learnt nothing—only taught—except reading some of Boole's book. I intend in a little while to put together Horner's method and send it you. It may be made very brief. I must *get up* the Quaternions—at my next leisure. I am sadly behind in them.'

[On the 15th of February De Morgan, acquitting himself of his promise, sends to Hamilton, in a letter of three sheets closely written, an exposition of Horner's Method. He gives the Hindoo rules for Evolution, Vieta's extension of them, and Briggs's completion of what is called Newton's approximation. He then introduces the Method by the following words about its author:—

'Horner, a most rare and peculiar genius for the organisation of a process, brought the whole case of $\phi x = a$ for every form of ϕx,

algebraical or not, under an organic process. He either did not see, or seeing did not value, his own place: when ϕx is integral and rational, his process makes him what *no other European inventor is*, the extender of the fundamental organic processes of arithmetic. He is the only inventor of his class whose name is known or whose country is certain.'

In an accompanying note he refers for further information to his articles 'Involution and Evolution' in the *Penny Cyclopædia* and *Supplement*. W. G. Horner died at Bath, Sept. 29, 1837.]

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *February* 20, 1854.

'The following little schemes are designed merely to show that I caught clearly your explanation of Horner's Process, no printed account of which has happened to be seen or at least examined by me for a long time, if ever; though I think that I *did* examine such an account, about twenty years ago, at the request of the late Secretary Drummond here—the inventor of the light so-called—who wished to have my opinion of the process. My recollection is that I reported favourably at the time, but never pursued the subject. It was by cube-roots and Cardan's Rule that I found a few months ago (partly as an exercise to show my boys) the real root of $x^3 - 4x^2 - 11x - 30 = 0$ to be nearly $= +6{\cdot}434247$, which I now see that Horner's method of calculation confirms. The work seems to run thus: . . . *February* 23rd.—I have recently solved (on Monday last) a linear and partial differential equation in bi-quaternions—the first, perhaps, of its kind (in point of time). Mr. Carmichael expressed lately to me a wish to know the general integral of $(iD_x + jD_y)\, U = 0$, and I have given him this solution (h being the *old* $\sqrt{-1}$): $U = (iD_x + jD_y)^{-1}\, 0 = (k + h)\,(D_y + hD_x)^{-1}\, 0 + (k - h)\,(D_y - hD_x)^{-1}\, 0$; = also $(1 + hk)\,\Phi(x + hy) + (1 - hk)\Psi(x - hy)$ where Φ, Ψ are two arbitrary bi-quaternions involving $hijk$ in any manner. Pray keep a note of *this*, for I think it an important germ. The existence of *two* arbitrary functions in this integral of an equation of the *first order* is a startling departure from the analogy of the older algebra. *That* I can't help any more than $ij = -ji$.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'OBSERVATORY, *March* 4, 1854.

'I accept your apology for a *primordinal* quaternion equation containing two arbitrary functions, namely, that it was not your doing. I have often heard inventors blamed for complexities which they had no hand in making, but found ready made in subjective nature. But arbitrary functions are queer fishes—for instance, $\frac{d^2z}{dx^2} = \frac{dz}{dt}$ is solved by one arbitrary function, if you take t as the principal letter, and by two really distinct ones if you take x—and the two are no more than co-extensive with the one, and the one is resolvable into the two.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *March* 7, 1854.

'Though I still hold to the *correctness* of my integral

$$U = (1 + hk)\Phi(x + hy) + (1 - hk)\Psi(x - hy) \ldots \text{(C)},$$

and of the equivalent form

$$U = (k + h)\,(D_y + hD_x)^{-1}\,0 + (k - h)\,(D_y - hD_x)^{-1}\,0 \ldots \text{(D)},$$

where h is the *old* $\sqrt{-1}$, as sent on February 20th to Mr. Carmichael ... yet I have had the satisfaction of discovering a transformation quite regularly flowing by the laws of my calculus from the form C or D, and involving only *one* arbitrary function, though *that* is of the *bi*-quaternionic kind, namely, the following :—

$$U = (iD_x + jD_y)^{-1}\,0 = e^{kxD_y}\Omega(y) \ldots \text{(E)}.$$

'The analogy to the history of $\frac{d^2z}{dx^2} = \frac{dz}{dt}$, though I am not *well* up in history at present, did not fail to occur to me.'

From the SAME *to the* SAME.

'OBSERVATORY, *March* 14, 1854.

'I this day took down from a shelf, where it had long tranquilly reposed, the second volume of Auguste Comte's *Cours de Philosophie Positive* (Paris, 1835), and read a considerable part

of it. My daughter looked over my shoulder during a short portion of the time so employed; and from the glance which she gave was led to say that she thought it must be a much less interesting work upon astronomy than Bailly's History of that science (Paris, 1805), which she has been slowly reading with me in the French. To *her*, of course, Comte's book would have been such, if she had found time and patience to read it. To *me*, however, who am, or was, something of a metaphysician, it is an extremely interesting composition, the atheism notwithstanding, which (I think) scarcely irritates me at all: for the author's sake I might naturally wish it to be otherwise; but as regards myself, in reading, his point of view and mine are too entirely remote from each other, on *such* subjects, to leave any practical chance of a collision. I think that I am reasonably tolerant of differences of opinion, in all things whatsoever; but suppose that it would be easier to get me into an angry mood (at least the commonly observed circumstances of human nature would favour that conjecture, though I have not actually experienced the occurrence in myself) on some point of interpretation of the thirty-nine articles of my own Church, than on the grand and fundamental question of the very *existence* of religion, as a rightful thing, which Comte so haughtily puts by. Regretting, therefore, for his *own* sake, his contempt of "*le fameux verset: Cœli enarrant gloriam Dei*," but not wishing to burn him for it, I turn to a few statements of his, respecting *astronomy*, in which he has ventured to be a little *mathematical;* and ask you whether you think that certain things, about to be particularised, are not blunders; and whether, if such, they can fairly be thrown upon the printers?

'Comte justly dwells on the importance of the two grand sorts of measurement in astronomy: the measurements, namely, of Time, and of Angle. I think, indeed, that if I took down Biot's *Astronomie Physique* (for example) from my shelves, I should find the same distinction and enumeration made, and dwelt upon; but let that pass, for the present. Well, Comte proceeds to speak of Galileo and of Huyghens, in connexion with Pendulums and the measurement of time; and here let me acknowledge that I greatly admire in Comte his *capacity of admiration*, and the earnest praise which he gives to some great men—for instance, to the two just mentioned, and to others, among whom I may name Fourier (the

subject of a sonnet of my own). But please to turn to the account which Comte puts forward of the Isochronism of the Cycloidal Pendulum, and the curious metamorphosis which under his hands is undergone by the doctrine of the equality in time of the small and large swings thereof, in pages 57, 58, of the volume above referred to. Or rather, to save you trouble, and because you may not happen to have the book in question within your library at present, permit me to quote the passage. "Car il en résultait" (he means from Galileo's discoveries) "nécessairement que la vitesse d'un poids qui descend suivant une courbe verticale décroît à mesure qu'il s'approche du point le plus bas, en raison du sinus de l'inclinaison horizontale de chaque élément parcouru: de sorte qu'on pouvait aisément concevoir, que, par une forme convenable de la courbe, l'isochronisme des oscillations serait obtenu si le ralentissement se trouvait, en chaque point, compenser exactement la diminution de l'arc à décrire." Can you stretch your intellectual charity so far as to believe that the author of the foregoing sentence knew what he was talking about? Can you believe that at the time he knew, or at all events that he remembered, the distinction between velocitas and vis acceleratrix? Not that *I* pretend to have read the celebrated treatise "De Horologio Oscillatorio": *that* is, I trust, a pleasure to come. But some elementary dynamical notions are not so easily forgotten.

'Again, just turn to page 76 of the volume referred to, or let me quote a sentence from it here, respecting astronomical refractions:—"L'usage réel de ces formules est tellement peu fondamental désormais, dans les déterminations de ce genre, que l'on regarde comme presque indifférent, par exemple, de supposer la réfraction proportionelle au sinus ou à la tangente de la distance zénithale apparente. Si des tables qu'on présente comme fondées sur des hypothèses mathématiquement aussi différentes coincident néanmoins, en réalité, d'une manière presque absolue, jusqu' à 80° du zénith, c'st sans doute parce que ces hypothèses n'ont pas joué un rôle effectif bien important dans leur construction."

'As to this assertion of Comte, that astronomers treat it as almost indifferent, or at least as an unimportant question, whether they should regard the refraction as proportional to the sine, or to the tangent, of the zenith distance, I can only *conjecture* that he may have chanced to cast his eye on certain tables appended

to the *Connaissance des Temps*, for the valeurs de logar. $\left(\frac{\text{cosinus hauteur vraie}}{\text{cosinus hauteur apparente}}\right)$; some degree of constancy of course existing, in the addition for refraction to the logarithmic sine of the observed zenith distance. It would seem that Comte did not know, or had forgotten, that $d \log \sin z = \frac{dz}{\tan z}$.

'But is it becoming that a man, ignorant of, or forgetting, a thing so *very* elementary, should lecture the human race on *mathematical* subjects? and get a certain amount of credit from clever readers in general, as if he were *admitted* to be a mathematician? and even to be one of a somewhat high order? He speaks of final causes (p. 37, *note*), as having been praised by astronomers on anatomical grounds, and by anatomists on astronomical ones. Has not Comte himself been deferred to by physiologists as if he were a mathematician, which he certainly *is not*, and by mathematicians as if he were a physiologist, which I strongly doubt him to be? I mean, of course, in an *accurate* sense of the word, for certainly I do not pretend to be a judge myself of the degree in which he may *approach* to being so.

'(*March* 16*th*, 1854, *Thursday evening*).—Just starting for Dublin, to attend the annual and stated meeting of the Royal Irish Academy, of the Council of which I was elected to be a member, at least a quarter of a century ago. I add to what is above written, the doubt whether it was a *printer's* mistake, the confusion between sine and secant in the sentence of page 110 :—" Car, la sécante du demi-diamètre apparent d'un corps sphérique est évidemment égale au rapport entre son rayon réel et sa distance à l'œil." These specimens, which I could easily multiply, may suffice to justify a profound distrust of Auguste Comte, wherever he may venture to speak as a mathematician. But his vast *general* ability, and that personal intimacy with the great Fourier, which I most willingly take his own word for his having enjoyed, must always give an interest to his *views* on any subject of pure or applied mathematics.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*March* 26, 1854.

'I never made a study of Comte: his awful verbosity and great self-sufficiency frightened me. I should hardly have thought him so poor as your extracts prove him to be. There is nothing to justify his absurd confusion between velocity and acceleration in Huyghens, whom I have just looked at again. And as to his refraction, I am utterly at a loss to make him out, as to what he means.

'A man who pronounces the results of what man can fathom, or think that he fathoms, to be a final system of knowledge, is either so much above or below consideration that he will get none from me, unless, in the first case, he brings divine authority and miracles with him, which would be rather suicidal. An anti-supernatural revelation, to be established by miracles, would be worse than the sophism of Epimenides the Cretan.'

[The correspondence from De Morgan's letter of April 18 to his letter of May 1 is chiefly concerned with Dupin's well-known theorem regarding the lines of curvature on a system of orthogonal surfaces. De Morgan begins by calling Dupin's proof "diabolical," and giving one of his own which he had sent to the *Dublin and Cambridge Journal*. Hamilton (*April* 20) derives a quaternion proof, for which (*April* 21) he claims both "conciseness" and "clearness." He returns (*April* 24) to De Morgan's, "with great diffidence venturing to object to any analysis of yours," but stating firmly his objections, and congratulating himself on finding Dupin to be in agreement with him. Repeating this (*April* 25), he gives additional illustrations:—"All this may show that it has not been without giving the subject some careful, and indeed anxious consideration—since it involves some difference from *you*—that I have ventured to deny the truth of your two separate equations, while yet I admit (and contend for) the truth of the final combinations." De Morgan (*April* 27) thanks Hamilton for his criticism:—"There is no occasion for apologies: if it be wrong it is wrong.

I have this day received two letters—one from an Irish Geometer, apologising for telling me he thinks me wrong, and another *from a Quaker* apologising for *using the phraseology of his own Society*. Fact, I assure you. Shades of Barclay and Penn, and shade of Berkeley, too, if you come to that. Remember in future that I am as thick-skinned as the crust of the earth on all matters of opposition to any view of mine." (*April* 29).—"You are right about the point in Dupin. I have heard of a man who used to say that anybody who was right when he was wrong took an unfair advantage of him." (*May* 1).—"The correction of my mistake about Dupin's Theorem brings my demonstration in fact to his." Referring to demonstrations in two different parts of Dupin's work, he says:—"I had not seen except the earlier one." "I think this [the latter] demonstration must have slipped notice." A Paper by Hamilton *On the celebrated theorem of Dupin*, is printed in the *Proceedings* of the R. I. Academy, vol. vi. pp. 86-88, May 8, 1854. I give some extracts of interest from this correspondence]:—

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 18, 1854.

'Dupin's proof is diabolical. You must poke native genius into infinitesimals of the second order in a most distressing manner; and all the other proofs are dreadfully—not synthetical, but constructive—that is to say, they come well from a person who knows the theorem, but have not the least smack of inquiry.

'N. B.—The words synthetical and analytical are often used where *dogmatical* and *inquisitive* would be better substituted . . . I hope you are Eastering to your satisfaction, without any qualms from the name of the feast. Perhaps you do not know whence it was derived. A Quaker would be more shocked at it than at Wednesday.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 24, 1854.

'That proof (*Développements de Géométrie*, Ch. Dupin: Paris, 1813, p. 526), so far as I have glanced at it, seems to be one of a

discoverer rather than of a teacher; and to it, as to a large part of my own book, I should wish a powerful hydraulic compression to be applied, as an assistance in favour of future readers. But do not *you*, who have so much originality of your own, recognise the genius of Dupin? Indeed I know that it is unreasonable and unnatural to think otherwise of you. You may dislike (even *diabolically*) this or that *proof* of his: but he would be, I think, with you "le bon diable," if it fell to you to assign him a place for the evening among a mathematical company of which you were the head.'

'(*April* 29).—You say that you are not thin-skinned; or rather that you are "as thick-skinned as the crust of the earth on all matters of opposition to any view of yours." It may amuse you to know that in some old conversation of mine with Charles Graves, he said to me "De Morgan is *not* thin-skinned." Whereto I replied "De Morgan is a Hippopotamus!" So my dear Hippopotamus, I am, as ever, yours, &c.

'P. S.—I won't call you "Crust-of-the Earth."'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 29, 1854.

'Dupin has made his proof appear very long. But he writes evidently in the early day of the subject. Think of his noticing $1+pp'+qq'=0$ as having been given by Monge and by Lagrange —and stating where. We should as soon refer to Leibnitz for the subtangent of a curve. I fancy few look as far on into Dupin's book. When I referred to him as diabolical I meant his former demonstration, p. 239. But I admit he is a decent devil in mathematics.'

'(*May* 1).—I intend, when I become old and venerable, to write some letters to young mathematicians about various subjects, including insensibility to attack of all orders—priority-anxieties, &c. &c. I shall remember to have an Hippopotamus neatly vignetted for the title-page.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 25, 1854.

'If ever you shall turn your attention to the Calculus of Quaternions, I am convinced that you will do it much good. I

hope you admit that such a "Calculus" *exists;* and that such an expression for Dupin's theorem as I lately gave you is (for example) *intelligible* to persons (such as there are several in my own University) who have received or acquired a certain degree of training. . . .'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 29, 1854.

'The Calculus of Quaternions is most certainly a calculus. I am afraid I shall never be actually engaged in it as a matter of speculation. All I do arises directly out of teaching, and has in some way or other reference to what can be brought before a class, and especially with a view to mathematics as a discipline of the mind. I have never had time for more than naturally arises out of my occupations. In each year I teach the *whole* of mathematics —from Euclid, Prop. I., and $a+b=b+a$, and arithmetical rules, up to definite integrals, the method of least squares, and the like— in four different classes, each meeting three times a-week. So that unless a question begins in something relative to teaching it does not fix my attention. . . . I have at last become fully satisfied that the language and idea of infinitesimals should be used in the most elementary instruction—under all safeguards of course. I intend to write something on the rise and progress of this idea—and the history of it is curious.'

[On the 6th of May Hamilton sends to De Morgan *Short Notes on the* Calculus *of Quaternions*, the object of which was to show "how rules of *calculation* with quaternions would look when geometrical interpretation was kept quite in the background." These "Notes" fill five sheets of closely-written note-paper, and have value as possessing a certain completeness. On the 12th of May he writes again, saying that when he came to consider what was on the side of calculation the simplest way of proving the *modular theorem*, a theorem extending to the *octaves* of J. T. Graves and Mr. Cayley, he was led into a train of investigation interesting at least to himself. This investigation he then sets forth; but, as he soon afterwards communicated it to the *Philosophical Magazine*, it

is here omitted. The next following letters refer to an application of quaternions to physical science which has been mentioned, *supra*, page 5. It has been dealt with in Tait's *Elementary Treatise on Quaternions*, §§ 428 . . .]

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 25, 1854.

'Your note has been received, and some things in it shall perhaps be noticed more fully hereafter. Meanwhile, I am in a mood to tell you of something new, before my first interest in it shall have evaporated, and my mind turned to other matters. I must bring in quaternions (Hamlet *won't* let himself be left out), but shall assume little more than that a quaternion

$$Q = \omega + \rho = SQ + VQ = \text{scalar plus vector},$$

where the scalar ω, or SQ, is a positive or negative *number*, while the vector ρ, or VQ, is by me usually constructed as a *directed right line* in tridimensional space. You will also allow me to assume that, in a certain definite sense, the product or quotient of two vectors is generally a quaternion, and will allow me to difference and differentiate.

'Well, I sought this morning to translate into my own notation the law of attractive or repulsive action (say f), of one element ds of a current on another element ds' of the same or of another current, which was discovered by Ampère, and by him expressed under the form (*Théorie des Phénomènes Électrodynamiques*, p. 217, &c.).

$$(f =) - \frac{2ii'dd'\sqrt{r}}{\sqrt{r}}, \text{ or } - 2ii'r^{-\frac{1}{2}}\,dd'.\,r^{-\frac{1}{2}}; \qquad (1)$$

the respective intensities of the elements being i, i', and their rectilinear distance r.

'I took ρ, ρ' for the *vectors* of the beginnings of the elements (drawn from an arbitrary origin), and therefore naturally denoted the directed elements themselves by $d\rho$, $d\rho'$; while $\Delta\rho = \rho' - \rho$ expressed the directed line from first to second; so that, on my principles,

$$d\rho^2 = -ds^2,\ d\rho'^2 = -ds'^2,\ \Delta\rho^2 = -r^2.$$

I was to pick out the part involving $d\rho$ and $d\rho'$ each in the 1st

dimension, from the development of $\{-(\Delta\rho + d\Delta\rho)^2\}^{\frac{1}{2}}$; and then (treating the intensities of the currents as each = 1) was to multiply the part so found by $-2(-\Delta\rho^2)^{-\frac{1}{2}}$, in order to get the required translation of Ampère's law. Or I was to select, by the condition mentioned, the proper part of the expansion of

$$-2\left(1+2S\frac{d\Delta\rho}{\Delta\rho}+\frac{d\Delta\rho^2}{\Delta\rho^2}\right)^{+\frac{1}{2}};$$

or the proper portion of this part thereof,

$$-\frac{d\Delta\rho^2}{2\Delta\rho^2}+\frac{3}{4}\left(S\frac{d\Delta\rho}{\Delta\rho}\right)^2;$$

or of

$$-\frac{(d\rho'-d\rho)^2}{2\Delta\rho^2}+\frac{3}{4}\left(S\frac{d\rho'}{\Delta\rho}-S\frac{d\rho}{\Delta\rho}\right)^2.$$

This part, or selection portion, is

$$f=\frac{S.d\rho d\rho'}{\Delta\rho^2}-\frac{3}{2}S\frac{d\rho}{\Delta\rho}S\frac{d\rho'}{\Delta\rho}; \qquad (2)$$

which accordingly I found to agree, through spherical trigonometry, with a perhaps better known formula of Ampère (quoted by De la Rive, and investigated by Murphy, &c.),

$$r^{-2}dsds'(\sin\theta\sin\theta'\cos\omega-\tfrac{1}{2}\cos\theta\cos\theta'),$$

when θ, θ' are the angles made by the elements with their connecting line r, and ω is the dihedral angle with that line for edge. So far, all is mere *practice* in my *calculus;* and you may say the same of these transformations of the expression (2),

$$f=-\tfrac{1}{2}S\left(\frac{d\rho}{\Delta\rho}\frac{d\rho'}{\Delta\rho}+V\frac{d\rho}{\Delta\rho}V\frac{d\rho'}{\Delta\rho}\right), \qquad (3)$$

$$f=-\tfrac{1}{2}S\left(\frac{d\rho\,d\rho'}{\Delta\rho^2}+3V\frac{d\rho}{\Delta\rho}V\frac{d\rho'}{\Delta\rho}\right), \qquad (4)$$

But you must know that I have been for more than ten years haunted with visions, or amused by notions, of some future application of the *Calculus of Quaternions* to *Nature*, as furnishing a Calculus of Polarities. (See my printed letter of Oct. 17th, 1843, to J. T. G., *Philosophical Magazine*, Supplement to December number for 1844, p. 490). More definitely, I have often stated, to Lloyd and others in conversation, my expectation that it would

be found possible to *express two connected but diverse physical laws* by means of *one common quaternion;* and Faraday may possibly remember my chat with him at Cambridge, in 1845, upon the subject of the analogy of the products of ijk, to the laws of electrical currents ($ij = +k, ji = -k$, corresponding to phenomena of electricity (see British Association *Report*, for 1845).

'Knowing this little bit of my own history, you will not be surprised that I sought to realize my old expectation to-day, after having translated Ampère's law into the notations of the Calculus of Quaternions. Having expressed his *attractive force* between two elements, as the *scalar part* of a quaternion,

$$f = SQ, \tag{5}$$

where Q may have either of the two forms given by (3) and (4)—and indeed others also, which I thought less proper for my purpose—my old conjecture led me to surmise that there might be *some directive force*, ϕ, perhaps an axis of a couple, perhaps a magnetic element, or what else deponent sayeth not, which should be expressed as the *vector part* of the *same quaternion*, Q; in such a way that

$$\phi = VQ. \tag{6}$$

The form which (3) would give for Q was at first tried; but I found that it had, what appeared to me to be inconsistent with the law of action and re-action, the property of *not* changing to its own *conjugate* quaternion, KQ, when ρ and ρ' were interchanged. From this fault the quaternion suggested by the formula (4) is free; and accordingly I assumed

$$Q = -\tfrac{1}{2}\left(\frac{d\rho\, d\rho'}{\Delta\rho^2} + 3V\frac{d\rho}{\Delta\rho}\,V\frac{d\rho'}{\Delta\rho}\right); \tag{7}$$

and proceeded to *try*, as a mathematical experiment, whether my old conjecture, expressed by the recent equation (6), might not lead me to the re-discovery of some known law of nature, or at least to some result identifiable with such a law.

'I soon found, by combining (6) and (7), according to the rules of my calculus, the expression

$$\phi = \tfrac{1}{2}(3\nu S\,.\,\nu\lambda\lambda' - \nu^2 V.\,\lambda\lambda'); \tag{8}$$

after having written, for conciseness,

$$\lambda = d\rho,\ \lambda' = d\rho',\ \nu = \Delta\rho^{-1}, \tag{9}$$

making also

$$V.\lambda\lambda = \mu, \tag{10}$$

we have this somewhat shortened expression,

$$\phi = \tfrac{1}{2}(3\nu S.\ \nu\mu - \nu^2\mu), \tag{11}$$

or (by the rules of this calculus),

$$\phi = \nu(S + \tfrac{1}{2}V).\,\mu\nu. \tag{12}$$

Up to this stage, I assure you that no recollection of anything about terrestrial magnetism (respecting which I know very little) had in *any* degree consciously influenced my transformations. So much had those things been out of my head that I was obliged to ask my son Archy whether in fact the Dip was greater than the Latitude, or the Latitude than the Dip. But having seen that equation (12) gave

$$\frac{V}{S}.\,\nu^{-1}\phi = \frac{1}{2}\frac{V}{S}.\,\mu\nu, \tag{13}$$

I perceived that (by the principles of my *Lectures*)

$$\tan\mu\nu = 2\tan\nu\phi\,; \tag{14}$$

and this not only *suggested* things about dip, &c., though very vaguely remembered, but led me to see that, for a first approximation, I was to assimilate the joining line, $\nu^{-1} = \Delta\rho$, to the *vertical;* and to compare the line μ, perpendicular to the elements of the current, to the *magnetic axis* of the earth; and finally to consider my conjectured *line of directive force*, ϕ, as being physically represented by the *dipping needle.* At least it is so in *direction*, even when *plane* is taken into account; but I thought it *very likely* (such was my faith in the quaternions), that the formula (12) for ϕ would be found to represent also the *intensity* of terrestrial magnetism; to the same order of approximation, I mean, as the formula,

$$\tan(\text{dip}) = 2\tan(\text{mag. lat.}).$$

By taking the *tensor* of ϕ, using (11) rather than (12), I inferred this magnetic *intensity** to be proportional to

$$\sqrt{1 + 3\sin l^2};$$

*It was *partly* through a *foresight* of *some such* use as this that I ventured to introduce the word "Tensor," in my writings.

because the formula (11) gives (on my hypothesis, *intensity* =)

$$T\phi = \tfrac{1}{2} T\nu^2 T\mu \{1 + 3(SU \cdot \nu\mu)^2\}^{\frac{1}{2}}. \qquad (15)$$

(I use here the symbols T and U, to which I am accustomed).

'(RECAPITULATION.—$f = SQ$ = Ampère's *attractive force* between the elements $d\rho$, $d\rho'$, of a current, with intensities each = 1, and separated by the interval $\Delta\rho$; $\phi = VQ = \nu\,(S + \frac{1}{2}V)\,\mu\nu$, when $\nu = \Delta\rho^{-1}$, $\mu = V \cdot d\rho\, d\rho'$; and ϕ = a (new?) sort of *directive force*, which seems to be nearly represented by the *dipping needle*, if ν be treated as the vertical at the place, and μ as the magnetic axis of the earth).

'When I had got so far I conjured my son Archy, after his returning from our parish church—(I am ashamed to say that I had got *caught* in this investigation early, and did not observe the hours, till it was too late for me to shave and walk)—to hunt out all the books in this house bearing any relation to terrestrial magnetism, and to search among them whether any law of intensity, of the form

$$\text{const} \times \sqrt{1 + 3\,(\text{sin. mag. lat})^2}, \qquad (16)$$

was recognized. He had to perform duties of hospitality to a party from this neighbourhood, and could not immediately find any information for me of the kind required. I went roaming about the house; hunted out Biot's *Physique*, 3rd vol.; Turner's *Chemistry;* Kane's; a book of Hassenfratz; and several others, without being able to find anything bearing on the point. At last I lit on Christie's *Report on the Magnetism of the Earth*, embodied in the Report of the B. A., for 1833, in which, at page 121, I found the formulæ,

$$\tan\delta = 2\tan\lambda,$$

$$I = \frac{m}{2}\sqrt{3\sin^2\lambda + 1};$$

deduced, it is true, from "the hypothesis of two magnetic poles not far removed from the centre of the earth," and very far inferior in accuracy to what is given by the Gaussian constants; but yet having a very encouraging analogy to my own theoretical results, especially as my investigation (it must always be remembered) relates only to *two linear elements.*

'On the whole, I think that it is not without some just cause that I propose to call the quaternion

$$Q = -\tfrac{1}{2}\left(\frac{d\rho\, d\rho'}{\Delta\rho^2} + 3V\frac{d\rho}{\Delta\rho}V\frac{d\rho'}{\Delta\rho}\right),$$

the "ELECTRO-MAGNETIC QUATERNION," of, or resulting from, the *two linear elements*, $d\rho$ and $d\rho'$, separated by the interval $\Delta\rho$.

'You see that the notion of "*current*" is quite eliminated here, though the mere *word* has been retained. The $d\rho$ and $d\rho'$ may be *directed tensions* or elementary *axes of rotation*, or anything else which answers to a *directed and linear element* in space. I must confess that I am strongly tempted to BELIEVE, that a differential *action*, represented by the vector ϕ of my quaternion Q, EXISTS IN NATURE; but if experiments shall overthrow this opinion, it will still have been proved that my Calculus furnishes an *organ of* EXPRESSION, adapted to very complex phenomena."

From the SAME *to the* SAME.

'OBSERVATORY, *May* 27, 1854.

'Though I let a letter go off this morning, after causing a copy to be kept, which letter had been written two days ago, I feel much less sanguine than when writing it, about its having any *physical value at present;* even if, with modifications to be indicated by facts, it shall ever come to have any.

'The *scalar*, SQ, of the quaternion

$$Q = -\tfrac{1}{2}\left(\frac{d\rho\, d\rho'}{\Delta\rho^2} + 3V\frac{d\rho}{\Delta\rho}V\frac{d\rho'}{\Delta\rho}\right), \qquad (7)$$

undoubtedly represents Ampère's law of the attractive or repulsive action between two linear and directed elements of a current, or of currents, $d\rho$ and $d\rho'$, with the directed distance $\Delta\rho$. That is a mere mathematical fact of calculation. But you know that *any vector*, whatever, suppose κ, may be added to a quaternion Q, without changing its scalar part:

$$S(Q+\kappa) = SQ, \text{ if } S\kappa = 0.$$

My choice of the form (7) must therefore be admitted to be

eminently conjectural, *even if* the principle to which I still cling, that *some* quaternion form Q, of a simple kind, exists, which by its scalar part SQ expresses *one* mode of physical action, and by its vector part VQ expresses *another* connected and connate mode of force or influence in nature.

'On the whole, without having perceived any mathematical error in what I lately wrote, I *withdraw* the epithet "Electro-Magnetic," as assuming too much, on the *physical* side, for my recent quaternion Q. But though I cannot hope that your own avocations, and researches, have allowed you as yet to catch much more than the *spirit* of my calculus (what a splendid quaternionist you would become, if you ever really set about it!), I think that if you have looked, even hastily, over my letter of May 25th, you will have seen that I do not speak *at random*, when I assert that this one short expression of mine for Q contains at once, by its scalar part the Ampèrian function

$$f = r^{-2} ds\, ds' (\sin\theta \sin\theta' \cos\omega - \tfrac{1}{2} \cos\theta \cos\theta'),$$

and by its vector part the three other laws:—1st, of a certain resultant axis ϕ being *in the same plane* with the connecting line $\rho\Delta$ and the common perpendicular to the elements $d\rho$, $d\rho'$; 2nd, of the *tangent* of a certain *dip* (δ) being *twice* the tangent of a certain *latitude* (λ); and 3rd, of the *intensity* varying as $\sqrt{1 + 3 \sin \lambda^2}$, when the elements are otherwise given.

'The analogy to magnetism is perhaps very vague—indeed I suspect it to be so; but if *one* small quaternion can *mean so much*, may not something be hoped from some future shake of Lord Burleigh's head? (*Vide* Sheridan's *Critic*).

'I have, however, the common sense (sometimes) to admit that no ingenuity of speculation can dispense with an appeal to *facts*. And what I am at present extremely curious to know is whether facts are decidedly against what seems to me a natural supposition, that *two rectangular and rectilinear* (*and non-intersecting*) *conducting wires*, though *not attracting nor repelling each other*, may have *some tendency to assume parallel positions*:* or may be the occasion of

* I have since seen that this rotatory tendency *does* follow from Ampère's *attraction*, though not for the elements at the very extremities of the common perpendicular; and on expressing and integrating the moments of rotation have got results which I have the satisfaction of finding to be coincident with his

some other DIRECTIVE FORCE arising, *before attraction* or repulsion begins. Oersted's discovery (I have met Oersted, and he has given me some very pretty and rather poetical German papers) seems to be *almost* a proof that this is so: but my wish is to eliminate, if possible, *magnetism*, at first, as an eminently *complex* phenomenon.'

'OBSERVATORY, *June* 1, 1854.

'P.S.—This morning's post brings me a letter from Lloyd, influenced no doubt by our old friendship, but containing a far greater degree of encouragement than I had expected to receive, as to what I had called the "Electro-Magnetic Quaternion."*

'Hope to write soon on something else.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*July* 10, 1854.

'I have been occupied with the ending of my Session, and all sorts of things besides, and have, I believe, to acknowledge two notes of yours, mostly on quaternions, which you are pushing into the very heaven of heavens. But so much the better; get on as far as ever you can, but do not expect anybody to plow with your heifer yet awhile—it is quite enough if anyone can read your riddle. I see you have unmasked a battery in the *Philosophical Magazine* on the subject of your notes.

'What are you doing in the meanwhile as to things in general? Quaternions cannot occupy your whole existence.

'I am just resting from the preparation of a lecture which I am to let off to-morrow at this exhibition of educational objects, which the Society of Arts has got up in London. I take the connexion of mathematics and logic in education. Remember this, the rule for giving an extempore lecture is—let the mind rest from the subject entirely for an interval preceding the lecture, after the notes are prepared; the thoughts will ferment without your knowing it,

(see especially his page 86). In short, this little speculation, which *as such* I abandon for the present, has led me to learn more of the precise nature of his beautiful theory, in what may be called a *few hours*, than I had done in my *life* before.

* *Supra*, p. 5.

and enter into new combinations; but if you keep the mind active upon the subject up to the moment, the subject will not ferment but stupefy. Whewell gave an introductory to the whole course to-day, and exhibited Stevinus actually in wood and metal, which it happened I never saw except on paper.*

'Did I ever ask you about the following work?:—

'"Second volume of the Instructions given in the drawing school established by the Dublin Society . . . 1768 . . . under the direction of Joseph Fenn, heretofore Professor of Philosophy in the University of Nantes," Dublin, 1772, 4to? It uses the notation of the differential calculus, not that of fluxions. Do make it a question at the R. I. A., for the honour of Ireland, what this work was.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 14, 1854.

'You acknowledged, in a pleasant note† about the four dimensions of a blue-book, and of quaternions (or of a book about them), the only letter of any real or supposed importance, which I can remember writing to you since our Dupin correspondence. I refer to one dated May 12th, of which a copy was preserved, and which I have since made in part the basis of a communication to the *Philosophical Magazine*, as I was glad to see that you on your part had lately printed some notices of things, respecting which you had written to me. But at this moment I cannot stop to look for any letter, book, or memorandum.

'Having so long procrastinated about returning your Mourey, I wished to retain some notes of the work, which might assist my own memory, and enable me perhaps to write a little to you about it, when it shall be in your hands again, as I really hope it soon will be, for I have lately made nearly as full an abstract of the book, in one of my own manuscript volumes, as I wish to have at hand. In fact between extracts (copied in the French, for practice), abridgments, and comments of my own, I have already filled fifteen large pages of such a volume, and got as far as his solution of a cubic, so that not much more remains. I should like to consider,

* Here is inserted in the letter a figure of a triangle with a horizontal base and a closed chain slung round it: referring to the argument of Stevinus (1548–1620) in proof of the parallelogram of forces, that if that proposition were not true such a chain would perpetually keep moving round and round.

† [This note is missing.]

if I can spare some quiet hours for it, his proof (or alleged proof) that every algebraic equation has a root; but I care more for his conceptions, definitions, and notations, which I think that I now perfectly understand, than for such applications of his theory. What he says about version, versors, &c., *might* no doubt have set me on the track of the quaternions, if I had seen his book as early as I did Warren's; but I had not only formed my own general views, but had published the *name* and *sign* (i. e. *my* sign, U) of the *versor of a quaternion*, at least as early as July, 1846, in the *Philosophical Magazine*, if not elsewhere before that date; and at a time when I had (certainly) not seen, and (I think) not heard of, the work by Mourey. The *conception* of the quaternion, as a *geometrical operator*, which at once turns (or *verts*) and stretches (or *tends*) a line, was familiar to me at least as early as 1844, and I think that I made a communication to the R. I. A. to that effect, about the end of 1844, or beginning of 1845, besides what I had shown in 1843 about the connexion of *ijk* with spherical trigonometry. My Lectures, you know, are mainly founded on that view. Warren, I think, was shy of putting *rotation* prominently forward, if at all (but I must look into his book again); with Mourey it is quite a *key*, for the *plane*, as with me for *space*. . . .

'Meanwhile, though quite manifold enough as a writer to be entitled, if you please it, to forget your own inventions, I fancy that you are so much a tidier librarian than myself as to be able, more easily than I should be, to decide whether the following triplet-system (to which I attach no importance) is among those published by you. In the proof-sheet* before me there is a note to say that "I am not aware that [it] coincides with any of the triplet-forms of Professor De Morgan, or of Messrs. John and Charles Graves; but it is given here merely by way of illustration." The trinome in question being

$$P = s + \iota x + \kappa y, \tag{76}$$

when xyz are ordinary variables, but ι, κ, peculiar symbols, the formula which defines their properties is this,

$$(\iota x + \kappa y)(\iota x' + \kappa y') = (bx - ay)(bx' - ay') + (a\iota + b\kappa)(xy' - yx'); \tag{77}$$

when a and b are any two ordinary and algebraical constants.

* Of Paper *On Extensions of Quaternions*, in the *Philosophical Magazine*, for July, 1854.

The functions $z \pm (bx - ay)$ are linear moduli. If you recognise this as your own, just cry, Stop thief! or *Au voleur!* at once, and you will be in time. More about my other doings hereafter.'

From the SAME *to the* SAME.

'OBSERVATORY, *August* 11, 1854.

'If you know anyone who would like to undertake the deciphering of what is written on the other half of this sheet, you are at full liberty to show it. Indeed, I should be glad if you would consent to hold stakes, if a wager of a few pounds were to be made upon the subject; and still more if you were to take charge of a sealed packet, containing a precise description of the key, which, in the meanwhile, I state to be one entirely definite, and absolutely free from ambiguity.

'My vanity and inexperience in the matter would dispose me to name six months, as not too long a time for the discovery of that key from the annexed passages in cypher, to which I should cheerfully add others, to any reasonable extent.

'But perhaps I should only appear ridiculous by naming so long an interval; and I am not quite inclined to let my name be publicly mentioned in the matter at all. An insertion, with initials, in "Notes and Queries," has occurred to me. Meanwhile, should you consent to be a referee?'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*August* 12, 1854.

'I will be referee, stakeholder, anything you please but decipherer. I only know that in your *key* is I. O. U., which is often the way out of difficulties; but often also the way into them. Babbage is a great decipherer—really a good one;—propose it to him. . . .

'I have just been looking at a table of quarter squares up to 200,000, which has some chance of being printed. Perhaps you know nothing about tables of quarter squares; many mathematicians do not.

'*December* 15, 1854 . . . I hope you are well and thriving. Do you dig in your garden? Do you cultivate the potato? I never

knew till 1846 how much my comfort depends on the potato. I knew I liked them, and I knew I could do without many things, but I did not know that I really could hardly do without the potato. Therefore I have since that time considered that the Irish have hit the right root, and have made up my mind that when I must eat only one thing it must be the potato.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, near Dublin,
'*January* 3, 1855.

'It was *one* motive for my so *showering* letters on you some time ago that I knew well that if ever a *pause* in our correspondence should occur, I was unlikely to resume it with anything like the same spirit and vigour as before, at least till an interval, which might be considerable, should have passed; and *that*, without even the *slightest* degree of diminution of regard.

'I enjoyed very much the frequency and fulness of our correspondence while my volume on quaternions was in progress, and have noticed that one or two of my Dublin University acquaintances,* to whom I mentioned the circumstance of our often exchanging letters, could not restrain themselves from shaking their wise heads, and saying (at least within their hearts):—"We understand it now; you owe it all to De Morgan"!

'A man who is really a *very learned* one, in both western and eastern languages, and from whom I took some lessons in German long ago, has recently been seized with a desire to acquire a knowledge of mathematics, and does me the honour to wish that I should give him some friendly assistance. While he was taking a little luncheon here to-day he boasted that he had lately read no less than 100 pages of Wood's or Purdon's Algebra, and on my happening to show him what I had just received, the new number of the *Philosophical Magazine*, containing the Fourth Section of my

* Please not to take me as sneering at my own Alma Mater, even as regards Quaternions. The Board of T. C. D. at last took courage to discharge the *whole* of my bill with their printer; and really, though they may be rich, they have a great variety of claims upon them. And as to the *young* men, who are just coming into notice, they have here a great degree of acquaintance with my Calculus, as I have tested by examining many of them myself, and hearing others examined publicly by C. Graves.

"Extensions" of Quaternions, he asked me whether I thought that *he* could read it? Whereupon, with a grave countenance, I asked him, what answer did he *wish* me to return? He caught the fun, and said that he supposed that he must wait for another *year!*

'A certain De Morgan, if you happen to know him, said something like that once, about the quaternions themselves. But seriously, and without any flattery, I think that *you* would find those *Extensions* comparatively *easy* reading—easier, in fact, to you than my volume, because more condensed. I *could* send you a *few* copies, if you cared for them.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*January* 6, 1855.

'I am very glad to hear that Trinity College has behaved so handsomely. But I beg you to understand that I consider myself the party quizzed by your friends, whose hearts you undertake to read with reference to quaternions, and they would see why if they had to make *me* stand an examination in quaternions. As time rolls on I find I can teach with a given amount of vigour in a shorter time. My Sessional Course to one class is about 115 hours. A shorter demonstration of an essential theorem is time saved, and every session I gain an hour or two. By-and-bye I shall have time to introduce the *element*, perhaps the *elements* of quaternions, and then I must set to work in good earnest to teach myself.

'Sometimes, however, I find I must lengthen a subject a little bit. For instance, the other day, examining the proof that when $\frac{\phi x}{\psi x}$ is $\frac{0}{0}$ it becomes $\frac{\phi' x}{\psi' x}$, I found that I could not find a rigorous proof anywhere. For instance, as a specimen of reasoning in a circle

'Suppose that when

$$\left.\begin{array}{ll} \phi x = 0 & \phi' x = \infty \\ \psi x = 0 & \psi' x = \infty \end{array}\right\} \text{ when } x = a.$$

How do you prove that . . . .

$$\frac{\phi x}{\psi x} = \frac{\phi' x}{\psi' x}$$

from any common method? For the whole theorem must be

assumed, according to common methods, before you know how to deal with $\frac{\phi' x}{\psi' x}$. I did at last get a very rigorous proof, and brief enough, and you will get it too, if you think it worth while.

'Your friend of the eastern and western languages must wait a little.

'I think the time *may* come when *double* algebra will be the beginner's tool; and the quaternions will be where double algebra is now. The Lord knows what will come above the quaternions.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 9, 1855.

'Something lately recalled my attention to my old researches in dynamics, and on finding a copy of my Second Essay last night, after returning from a Meeting of the Royal Irish Academy, I set *to*, at a late hour, and never stopped till, cutting the pages with quite a feeling of *curiosity*, I got to the end—of course only skimming parts, but finding, with satisfaction and surprise, that I *understood* it all as I went on! for I had fears lest the quaternions should have driven all such things out of my head, almost beyond hope of recovery. This morning I resumed the reading with greater care, and may mention equation (53) in page 106 of the copy, which is paged like the *Philosophical Transactions*, from which it is taken, as one which stopped me for awhile, but of which I now fully see the demonstration.

'But perhaps I flatter myself too much in thinking that you may have *preserved* a copy of that Second Essay, if I gave you one, as I think that I must have done. You know probably that Professor Donkin of Oxford spoke highly of those dynamical researches of mine, in a Paper of his own, about a year ago, which now that I find I understand my *own* papers, I do not despair of reading. I *did* connect with quaternions the foundation of my own method, as long ago as 1845, but had since dismissed the subject from my thoughts.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*January* 15, 1855.

'You will amuse me at last if you go on in this manner. Do you pretend to forget that I wrote to you informing you that I had all your tracts except that identical record of Dynamics, that you thereupon promised me *several;* that you kept my tracts unbound, notwithstanding my occasional groans, duly posted, for at least a session of Parliament, and that you did then send me several? Had I any other consolation for being prevented from regularly reading the "discontinuous functions," and one or two other things (and prevented I was by all your Papers being tied up and put by until I could get the missing one), except the hope that remorse would haunt you to your dying day? And do you not now utterly destroy this consolation by talking to me about "if I gave you one, as I think I must have done"?

'You have been reading your own Papers, fearing that you would not understand them. I have read a Paper (but not on mathematics) before now, have said to myself, I perfectly agree with this man, he is a very sensible fellow, and have found out at last that it was an old Paper of my own I was reading, and very much flattered I was with my own unbiassed testimony to my own merits.

'I send you something about differential equations.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 29, 1855.

'What enchanted castle is your knighthood attacking that one never gets a word from you. I am reminded of you as follows:—

'You will probably see the new *Quarterly Journal of Mathematics*, and a theorem about roots in it. I have had a curious note to write about it, and having written, and re-written it, I send you the original draft, which you may burn when read, or before if you like. But if you happen to know anything about these lost methods, tell me where you got it. I am perfectly surprised to find a fundamental method of Newton, followed up by Lagrange,

unknown to everyone I ever come in contact with, whether in books, or speech, or writing.

'I have sent a Paper to Cambridge on the subject. Newton and Lagrange among them make out the following problems, that is, such as I can see it by their light:—

'1. Given the nature of a curve when x is infinitely small, that is, any singular point you like at the origin and any characters of the branches. Given also the nature when x is infinitely great. Assign your branches real or imaginary as you please. Required to know whether the conditions are compatible, and, in that case, to give the most general algebraic equation of the curve.

'2. Given an equation with a parcel of equal roots, and given certain infinitesimal alterations in the co-efficients, required to know what becomes of the equal roots, how many remain equal, how many are $(a \pm b\sqrt{-1})$-ed,—to invent a new participle.

'All this might have been done by Newton, Taylor, Stirling, Degen, Cramer, Lagrange. I think none of them *have* done it; it remains to see whether anyone else has.

'Who is Mr. Carmichael who has sent me a very nice book on the Calculus of Operations? I have seen his name in the Journals, but never heard who he was.

'Rouse out from the quaternions, and write me a line. Remember that I, who have studied biography, and especially looked at the psychology of inventive mathematicians, do positively make affidavit that if a man do not fallow, and shift courses, as they say, or used to say, in farming, he will put his head *out of heart*. If I had the power—supposing it true, as I hear, that you will not let quaternions alone for awhile—I would put you into the commissariat, and make you copy out Balaclava stores, for six months. How are you all?'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 2, 1855.

'I was very glad to get your note and historical sketch yesterday, and think of writing a rather long letter soon, perhaps to-day, after my present messenger (whom I cannot delay) is gone, but at this moment can only say that I was up all last night with the moon, and that my health and spirits are pretty good, notwithstanding a great deal of work got through during the last six months.'

From the SAME *to the* SAME.

'OBSERVATORY, *May* 4, 1855.

'I wrote a line or two just merely to acknowledge your kind note, reserving to myself to *answer* it afterwards, or at least to write a pretty long one in return, as this is likely to be. You and I are, I hope, men much too wise to be huffy; yet I had some fears lest I might have been so very negligent towards nearly everyone in the letter way, for some time past, that you and all might have said to yourselves, Well, if Hamilton won't write to us, we shan't write to him—that's all. However I had *some* correspondence, and a pretty brisk one too, with an old friend and his wife lately, who had asked me (by anticipation) to be godfather to an infant of theirs, and who induced me, after some reluctance, to consent. The boy is born, but not christened yet, nor have I seen him; for they are all at present residing in the North of Ireland, though likely to come soon to live within a walk of this place. I call the child "*him*," not "*it*," and though to be sure a person can't be sure of the *sex* of an unborn infant, yet I felt in this case, and described in a letter to the mother, what of course I have felt at the births of my own three children, namely, the strange investiture with *personality* which seems to take place when an expected baby is *born*. Whatever may be the feelings of the *mother* beforehand, I am conscious of scarcely thinking of the child *before* birth as more than an incident of *her* health. The *moment* afterwards, *he* or *she* becomes an independent human being, with powers and responsibilities, and character in germ, and felt by me to be so.

'A very odd and original lady, entirely unlike the one to whom I have been last alluding, had also lately a baby: such things you know will happen, at least in Ireland; and on my being asked to hand her in to dinner, at a party given by Colonel and Mrs. Larcom in this neighbourhood, when I met her for the first time in my life, she told me of this "young pagan," as she called him (or *it*, for I did not know the sex. *I* don't call newborn infants in these countries *pagans*); and she asked me to be a godfather, perhaps because I was so to a grandson of Wordsworth the Poet (who lately wrote to me an account of the death of his Aunt Dorothy), and because she is an admirer of Wordsworth. However, I

declined. But it seems that I have not fallen entirely out of favour thereby, for she paid me, on Saturday last, a visit of three hours and a-half, it being my *second* time of seeing her. You must observe, however, that I had made it a sort of open day, and had several other guests, including a troop of deaf and dumb boys, for whom, and for the others, Lady Hamilton, though prevented by a heavy cold from being disposed to appear herself, had laid out a comfortable luncheon, and allowed her daughter to be present. My visitress told me, as we drank a glass of wine to the health of her child, that he had been christened on the previous day, by a long baptismal name, or string of names, the two first of which are Oscar and Fingal! the third and fourth sounding to *me* as a tremendous descent, but I daresay she prefers them. You must know that I have been long acquainted with her husband, as a Member of the Royal Irish Academy, though he had not time to come with his wife, on her long and entertaining visit of the other day. She is quite a genius, and thoroughly aware of it.

'One thing she said, as I was conducting her upstairs to the Dome, and while she was professing to admire the house (which she *hoped* was a *haunted* one—my sister believed it to be so), was: "Let a woman be as clever as she may, there is no prize like this for *her!*"

'You may judge that my *spirits* have not suffered very much from my hard study. I began my last *long* letter to that other lady, by saying that Lady H. had pronounced me to be grown quite a *good boy* of late—so sociable and neighbourly—only she feared it was too good a thing to last! And in fact I *have* been paying more visits and attentions lately than usual to my neighbours hereabouts. I wonder how they tolerate me at all as an acquaintance, I take such and so long fits of locking myself up at times; but I believe that I am on pleasant terms with them all. And now comes something in which, though you may think that it sounds oddly in this connexion, *you* are interested, at least your curiosity is, for I can connect an answer to an inquiry of yours with an account of a morning call.

'My wife had managed to get a sort of open carriage made, last summer, very simple and modest indeed, but still a vast improvement on my old outside car, for herself, as I supposed when I was paying for it, but, as it has turned out, almost entirely for me,

since she prefers another sort of vehicle for her own i excursions. I felt a little *taken in*, at being expected t *myself*, but have gradually become reconciled to it, an it quite a comfortable thing.

'*May* 10, 1855.—I was intending to tell you, and ma a drive to which I lately treated Carmichael in that little and of our lunching together at the house of a grandso Brinkley, but am starting for the Zoological Gardens w daughter, and send you a bit of mathematics, in return for y

'All compliments from yours faithfully.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town

'*May* 16, 1855.

'Thanks for yours, and the mathematics. Of the making o theorems there is no end.

'I fancy that, by the law of our language, *it* is applicable to babies, without imputation of heathenism—and naturally, for who is to know whether a baby is *he* or *she*? I am always impelled to say *she*, such being in my head the pronoun for petticoats, short or long.

'N. B.—In this country, when an infant is deprived of the long limp part of its dress which hangs down, *it* is said, *it*, the baby, to be *shortened*, and mothers talk with great glee of the approach of this horribly-sounding operation, which seems as if Procrustes was to have the baby, and which always makes me start to hear of it, and gives me a turn, as they say, just as $4!$ for $1 . 2 . 3 . 4$, and $n!$ for $1 . 2 . 3 \ldots n$, startle me and shake my nerves.

'*I* had sponsors, and if ever I see them, which cannot now be on this side of Styx, I shall ask them, in a civil way, how they ventured to undertake that, when I grew up, I should believe all they promised I should believe, reason consenting or not. I once had a friend who really believed that, if the infant did not get him or herself confirmed, they would be liable—the sponsors—for his or her default. He took care never to answer for anybody.

'I am glad to hear that you are seeing the world, and trust, in spite of Lady Hamilton's prophecy, that it will last.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 11, 1855.

'I deserve the reproach which I feel to be conveyed in your sending me by post the pamphlet which I received just now, and which I have read through, from beginning to end, in answer to Mr. Lowe's remarks [*in re* Decimal Coinage], without any accompanying letter. You must know, or be informed, that I at least *began*, and I think nearly finished, several different letters to you within the last few months—please to return the note which I hope to enclose, written by Miss Edgeworth in relation to some similar conduct of mine to *her*—and then threw them aside, till they were lost among my many masses of papers. There *was*, however—at least I think there was—some *use* in your excoriating as you have done the Hon. Member for Kidderminster. Does he *feel*, do you suppose, that he has become a Marsyas? if I remember my mythology enough for even such an allusion. Comical and crushing as your criticism on him is, I have not felt it to be so humorous (the occasion was not worthy of so much humour) as that never-to-be-forgotten letter of Mrs. Flamsteed's, which you allowed me to see as one of Baily's papers, and which I have since talked of to some clergymen, ignoring for the moment the fact of their being "Reverends."

'P. S.—I have no copy of the enclosed letter from Miss Edgeworth, which Mrs. De Morgan may like to see, but I wish it to be returned. It is likely enough that I may at some time or other have mentioned the existence of this particular letter on account of the "flagrant" pun contained in it from one who professed to abominate punning; but I can't remember sending it to you before. If there be a slip or two of the *pen* (not of the heart, or mind) in the letter, you must observe that Miss Edgeworth was more than seventy when she wrote it—"not an *elderly* lady, but an *old* lady, Sir William Hamilton," as on some occasion I provoked her to say to me.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*July* 13, 1855.

'To make sure, I return your letters, for which my wife and I are much obliged, at once. . . . If by Miss Edgeworth's pun you

mean "mistaking the point," I almost doubt whether intended. I have found various plays on words in my ings, made without intention, as that a point of histo *annalized* before it could be *analysed*—that certain jokes a dressed with *current* sauce (*signatum præsenti notâ*). The the satire without a letter was no reminder, for I sent at once as soon as I received them, and whether any let due to me from the several units of the score did not head at the time. But I am well pleased at the result.

'I am seriously of opinion that pitching into a joker service. In the first place it warns those who have nothing say that squibbing is a game two can play at. In the secon points of an argument cling very fast to the popular mind presented in a joke. As to my friend Mr. Lowe knowing th is victimised, he will learn that in the private talk of the Hou which nobody is so much the friend of another as to ignore a against him. They are all rivals—friends and foes. I do know how Protestant clergy take jokes on serious subjects, but Roman C. priests rather encourage them than otherwise. Di ever give you the Roman (locally Roman) story of St. Peter a St. Joseph?

'Now for a pun.—Why does the House of Commons, which is elected by the people, show so little knowledge of the feelings of the lower classes, that it attempts measures from which it is beaten back by a tumult? *Answer.*—Because the House is elected by the sovereign people, and not by the shilling and six-penny people. . . .'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*September* 5, 1855.

'I make use of some of the vacation leisure to write this scribble: what it is to contain I have not the least idea at this moment. If you read Brewster's *Newton*, as you ought, try to get hold of a review which I wrote of it in the *North British* (August). You will then get a pretty fair idea of the present state of the points at issue between the oppugners and defenders of Newton's weak points. If you get hold of the *Journal des*

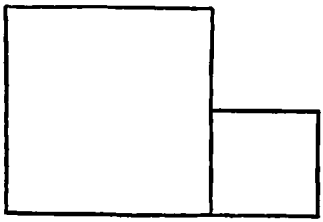
Savans for November and December, you will see Biot's account of the matter, he tells me.

'The decimal coinage and various assurance valuations have pretty well kept me from mathematics this vacation. But I find it an excellent thing to lie fallow, a practice I have recommended to you. A teacher who does not either fallow or sow another crop is sure to get into mere routine.

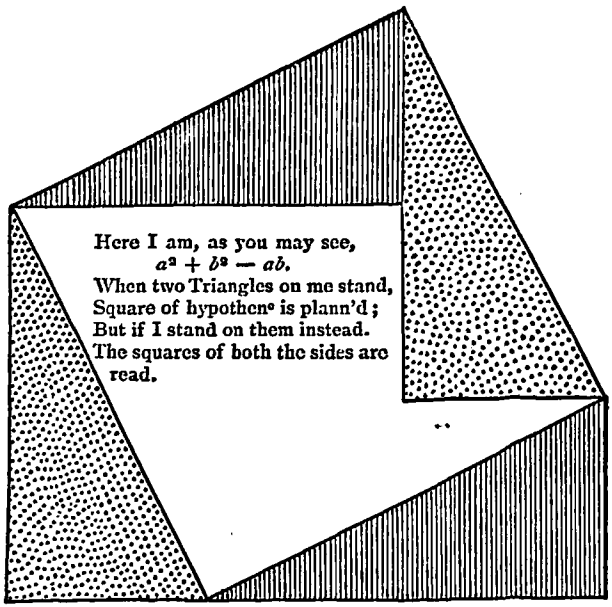
'I have been very much grieved during the last month by the loss of my friend Sheepshanks. You probably did not know him personally, and perhaps those from whom you were most likely to hear of him were not likely to give you a favourable impression. But a man who finds a dozen, or even half-a-dozen, such friends in his life is not badly off, as the world is constructed. I send you with this, if I get it in time, a sketch of mine which contains, I believe, the opinion of a solid mass of those who knew him well. It is fortunate that we do not know what is to happen, unless indeed we could know *onwards* a great deal further than the assertion is meant to include. I walked with him from the Council of the Astronomical Society ten days before his seizure, and took *au revoir* leave of him in the street.

'I have been looking at old ballads lately, English and Scotch. Is there a genuine Irish Ballad School *in English?* I mean of ballads springing from the people—not Moore, &c. I observe that the Irish melodies have *names* in English before Moore, but whether those names belong to English songs—songs in English—antecedent to Moore, I do not know. There are curious things about the history of popular ballads. The *music* of Auld Robin Gray was composed by a Somersetshire Rector, Mr. Leeves of Wrington (from whence Locke came), whom my family used to know when I was a boy. I remember being well laughed at by a party of Scotchmen for stating this, they affirming that it was a genuine old Scotch air, which they would not have said if they had known the Scotch gamut. But I see the fact now admitted, or rather put forward, in a recent collection of Scotch songs. The Irish are more musical, in the sense of originating melodies, than the Scotch, let the M'Sarcasm and O'Brallaghan question be settled how it may. Now that Mr. Duffy and anti-Saxon nationality are going to Australia, and other things can be attended to, can you not stir up some one to investigate and perpetuate the *composers?* I once

saw a collection of the airs melodized by Moore, taken do
the fingers of the harpers. They were loaded, *ad nausea*
gaudy ornament, showing that the ultimate simplicity
character is due to Sir J. Stevenson. And all my small inv
tions into music prove that simplicity is a product of civili
Alas! for romance and simple nature; but so it is. And so
mathematics too. Who would look for I. 47 in a more
mode than that of Euclid? Kellan
recently published a Paper of D
tions of the Square of the Hypothe
and Airy, regarding thereon, found
more simple mode, which he annou
to Kelland, myself, and others, may
in the following form:—



'Given two squares in a gnomon, required to cut the gnom
into *three* pieces, which, by motion of *translation* only, with
rotation, shall form into a third square. The process will
visible in the following figure, and from it may be deduced perha
the simplest demonstration of I. 47:—


Here I am, as you may see,
$a^2 + b^2 - ab$.
When two Triangles on me stand,
Square of hypothen^e is plann'd;
But if I stand on them instead.
The squares of both the sides are
read.

'Various proofs come near to this, but none which I can find comes up to it in simplicity.

'Placing the triangle in four different positions shows the proposition *anatomically*, without scalpel or nasty smell.

'I have been in my usual solitude, my family away at the sea; therefore, I have put all my things in order, and can find anything I want. *Quod erat desiderandum.*

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 2, 1855.

'I am in quite a mood to write to you about some mathematics again, though several letters of yours have scarcely been acknowledged. I had a pleasant time in Glasgow, and have been working here since my return.

'I regret the death of Sheepshanks, though I did not know him as you did.'

From the Same *to the* Same.

'Observatory, *October* 24, 1855.

'If I did not know you to be a very good-humoured correspondent I should scarcely venture to write to you now, at least without some apology which I should be tempted to postpone till the Greek Calends. I suppose you can be savage sometimes, but I have never found you so, and my impression is that you are too strong to be often cross. So I shall just chat away on things in general.

'To begin with the last thing: Have you seen a pamphlet by Wm. H. R. Jessop of Cambridge, on "A complete Decimal System of Money and Measures"? Do you think a book of that sort does much harm to the cause? Should you care to have my signature attached to some paper in favour of your views, which appear to me to be perfectly sound, on the subject of a decimal system of money, the pound being the unit? In general I am shy of signing papers, but in this case should have no hesitation. I will make you a present of Mr. Jessop's book, if you care for it, which I am so unpolite as to acknowledge that I do not. As I take you to be something of a "collector," which I am not, systematically, though I do sometimes buy a book merely because it is odd or rare, I have occasionally thought of offering to you some old pamphlets, which might happen to fill up a gap. About this, perhaps, I may write again, and at all events I shan't send you anything of that sort

without permission previously obtained. I give you, in the meantime, the first refusal of one *large* book, a half-bound collection of the "Morning Register" of Dublin, for 1825, which is intensely and bigotedly popish, but contains many interesting records of that time. Something bewitched me to pay ten shillings or thereabouts, a couple of years ago, for the book, and now I want to find somebody who will take it off my hands for *love!* for I am not seeking for any sort of pecuniary compensation. If you have no fancy for it, perhaps some one of my Roman Catholic friends (and some such I have, though a good Protestant myself) might like to include it in his collection; but I offer it (as a present) to you *first*. . . . Of my published papers I could easily spare you . . . those "Extensions" of Quaternions on which the last word has not yet been said.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*October* 29, 1855.

'I was just going to write to an Irishman, but I did not know who. You did not come into my head, because mathematician is your genus, and Irishman is but a differentia. But in came your letter and settled it. I have taken down an Irish chant (in Saxon) from the mouth of an American lady, called the "Fine Old Irish Gentleman," a burlesque imitation of the "Old English Gentleman." Is there any such song, in four line chant, current and received now? I never heard it, often as I have heard Irishmen sing national songs. Is it possible that such a thing may have died out in Ireland, and been preserved in the U. S. Can you apply to some songster, supposing you not one yourself?

'*Decimal Coinage.*—Do you know Lord Monteagle? He is one of the Commissioners who are to proceed to investigate. A public letter from you would be of great service. You might address it, even if you do not know him, as a witness, who is prevented by locality from attending in person. If you see the *National Review*, No. 2, there is an article of mine on the subject.

'Mr. Jessop knows nothing about it. His idea that weights and measures can be altered in a day, but that coinage must take time, is just the reverse of experience, and equally of *a priori* probability. He sent me his book.

'I shall like the old pamphlets—one is sure to find something. The *Morning Register* I could not use; you had better not delocalize it. Give it to some true blue Protestant. A couple of copies of the *Extensions* would be very acceptable.

'Quoad *Morning Register*. I can manage a Catholic my own way. A friend of mine (papist), after reading the article Galileo, in the *Supplement to the Penny Cyclopædia*, in which I have clearly shown that the decision of the *Inquisition* against Galileo never was considered the act of the *Church*, at the time, either by Catholics or Protestants, told me the Pope ought to write me a letter of thanks, and bantered me on my pro-Catholic argument. I replied to him that the Pope had written me a very civil letter, offering to make me a saint in heaven, or a Cardinal in Rome, regretting that he could not do both; and that I had replied, declining both alternatives, but informing his Holiness that if he would make me either a saint in Rome, or a Cardinal in heaven, I would accept, for then I should be the only one of my class. My friend stopped his banter, and circulated my reply widely among his sect, who, nine out of ten, enjoy a joke against the Church amazingly.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 1, 1855.

'If I were about to be examined on oath as to a complicated matter of *fact*, I should not approve of undergoing any sort of rehearsal beforehand: but in a question of deposing to an *opinion*, it seems right and wise to be very sure that one knows one's own mind first. I shall, therefore, without having recently read anything of yours on the Decimal Coinage, mention to you the chief points which I at present remember and adopt: if there be any one of them on which I do not agree with you, I request you to give me an opportunity of reconsidering my opinion, before I communicate with Lord Monteagle, with whom I have long enjoyed some degree of personal acquaintance, though not much cultivated by me for several years. . . .'

[Hamilton then lays down, in five propositions, what he considers to be the essential points to be observed in bringing about the desired change. De Morgan, in reply (*Nov.* 4), comments on

these *seriatim*, and concludes:—"There is nothing in your view substantially different from ours, except that you perhaps think there is more alteration of coinage required than is really necessary. . . . The great point is the *retention of the pound.* This being granted, all the rest is almost deduction. Can this country reckon in anything but pounds?" As the arguments and conclusions of De Morgan and Hamilton have already received sufficient publication elsewhere,* I have decided not to print in this volume the copious discussion of the subject contained in subsequent letters of this correspondence. In reference to another topic of the time, Hamilton had felt unreasonably aggrieved by observations of De Morgan on the conduct attributed to some Irish ladies at the trial of Mr. Carden for the attempted abduction of Miss Arbuthnot. They had clapped applause when on one occasion the Attorney-General, as conductor of the prosecution, had been defeated. Hamilton disputed the status of the clappers, and asserted that the sympathies of Irish ladies generally were on the side of Miss Arbuthnot. He was obliged on further information to modify his assertions respecting the rank of the persons commented on, but put forward the extenuating plea that they were displeased with the unfair and violent character of the prosecution by the Government. De Morgan, in final comment, writes as follows]:—

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*November* 4, 1855.

'. . . However, it stands, as I was convinced it did, that there are people in Ireland who admire the *voies de fait*, and have a sneaking kindness for romantic crimes. The Irish and Scotch both are defective in a quality in which the English abound—I call it legal-headedness. With us the Government might press a prosecution as hard as they pleased, so long as the crime they *put forward* was really the crime they were *prosecuting*. When they

* Compare, *supra*, pp. 93–95.

prosecuted Hone for *blasphemous* parodies, they failed, because all the world felt they were prosecuting for *political satire*, and that Hone might have parodied the catechism to the end of time, if he had let ministers alone. . . . If the Government had been strong against Carden, because he had been their opponent, &c., the English feeling would have been with the Irish. But I suspect the Government put forth its strength to secure a prosecution—and a real trial—knowing that the lower Irish, and a good many of the higher, are very lenient to the crimes of favourites . . .'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*March* 3, 1856.

'I send you the Fine Old Irish Gentleman, as I took it from the lips of a young lady from the United States. Whether it was born there, or is nothing but a returned emigrant, I do not know, and do not venture an opinion.

'I have added a verse, which is pure deduction from what precedes, and therefore is rather discovery than invention. If any of its phrases are too Saxon, they may be Hibernized to the fancy of the singer.

'So no more at present, for if I write five minutes more, I shall be obliged to alter the date, and since the additional verse is *discovery*, I may lose the priority thereby, so fast do people jump on each other's heels now-a-days.'

'"Now this fine old Irish gentleman had a || mighty curious knack
Of flourishing a tremendous great shillaly, and letting it fall down with a most un|| compromising whack.
But of most superior shindies you may take your oath, if you ever happen to be called upon for it, he very nearly || never had a lack,
And it's most natural, and not all surprising, to suppose that the fine old Irish mud was well ac||quainted with the back
Of this fine old Irish gentleman of the rale old stock.

'"Now this fine old Irish gentleman was once || out upon a spree
And as many a fine old Irish gentleman has done, and more by token will do to the end of time, he got about || as drunk as he could be.
His senses were completely mulvathered, and the consequence was that he could || neither hear nor see,
So they thought that he was stone-dead and gone entirely, and the best they could do would be to have him waked and || buried dacently,
Like a fine old Irish gentleman of the rale old stock.

'"So this fine old Irish gentleman was laid out upon a bed
With half a dozen candles at his heels, and two or three dozen less or ‖ more about his head.
But when the whiskey bottle was uncorked he could not stand it any longer, so he ‖ riz right up and said,
Says he, when such mighty fine stuff as that is going about, d'ye think ye'll find such a soft-headed ‖ fool as to be dead
In a fine old Irish gentleman of the rale old stock?"

'The following verse has been revealed to the transcriber, as representing the impenetrable future of the time when the preceding verses were written:—

'"Now what d'ye think t'was after all that sent the fine old Irish ‖ gentleman to wrack,
For the shillaly was his theory and practice both, and as to the drop of whiskey ye'll be puzzled to make it anything but meat, drink, fuel, and ‖ clothing to his back,
Oh! 'twas Mr. Commissioner Hargreave, devil encumber him, got the patch of potatoes into the Court, and ‖ sold it in a crack,
For he said 'twas a negative quantity, and there's never a Christian knows what he means, or whether he demeans himself to mean anything at all, but since that time there's been a ‖ melancholy lack
Of the fine old Irish gentlemen of the rale old school.'"

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, near Dublin,
'*June* 11, 1856.

'I have whole lots of things to write to you about, but let me first get off my mind a piece of spite against an author whom you (with justice) admire.

'A few years ago you recommended me to get Grant's *History of Physical Astronomy*. I have only recently acted on the suggestion, stimulated, perhaps, by receiving an account of the well-deserved honour paid to the author by the Council of the Astronomical Society. The book is very valuable, and very creditable to its composer. But your humble servant may be pardoned if he finds himself somewhat *amused* at the *title*, "History of Physical Astronomy, from the Earliest Ages to the middle of the Nineteenth Century," when he fails to observe any notice of the discoveries of Sir W. R. Hamilton in the theory of the Dynamics of the Heavens.

'Jacobi thought them of importance enough to deserve an elaborate commentary at his own hands, and of course I admit, though perhaps it is rather lately that I have really *felt*, how very richly *he* adorned the subject, by taking it up where I had left it. He seems to have considered my transformations of the *differential* equations of motion of a system,

$$\frac{d\eta}{dt} = \frac{\delta H}{\delta \varpi}, \quad \frac{d\varpi}{dt} = -\frac{\delta H}{\delta \eta}, \tag{A}$$

to be almost as important as my discovery of the *integrated* equations,

$$\varpi = \frac{\delta S}{\delta \eta}, \quad p = -\frac{\delta S}{\delta e}, \tag{B}$$

in my Second Essay on Dynamics, of which I could still place a copy or two at your disposal. It might be an affront to offer one to the author of the History! I heard Jacobi speak of me, at Manchester, in 1842, when I was sitting, by invitation, beside Dr. Peacock, the then President of the Mathematical Section of the British Association, as "le Lagrange de votre pays"; but Mr. Grant ignores my existence. I can bear it!

'Besides the notice by Professor Donkin of Oxford, who has been pleased to print, as the first sentence of his first Paper [in the *Transactions* of the R. S.] "On a Class of Differential Equations," &c., this passage:—"The Analytical Theory of Dynamics, as it exists at present, is due mainly to the labours of Lagrange, Poisson, Sir W. R. Hamilton, and Jacobi; whose researches on this subject present a series of discoveries hardly paralleled, for their elegance and importance, in any other branch of mathematics" (*Philosophical Transactions* for 1854)—I transcribe at this moment from a private copy—besides this very full and handsome recognition, by one who is fully competent to judge, and a slight, but favourable notice of one point of my theory of dynamics, by Mr. Cayley, in the *Philosophical Magazine* for the present month, I have received, rather lately, in a parcel of foreign books, a quarto volume, by M. Hoüel, of Alençon, entitled, "Thèses, presentées a la Faculté des Sciences de Paris, pour obtenir le Grade de Docteur ès Sciences" (at this moment I copy from a memorandum made by another person, the book itself having been lent by me to the governing Board of my own University), and of the two Theses in the

volume, one is entitled:—"Thèse de Mécanique: Sur l'integration des équations differentielles dans les problèmes de Mécanique" (almost entirely devoted to a study of my Papers, and of Jacobi's extensions of my results); and the other, "Thèse d'Astronomie: Application de la Méthode de M. Hamilton au calcul des perturbations de Jupiter." As I have not the book itself beside me at this moment, I shall merely say, from memory, that after correcting a large number of errors of detail, of Pontécoulant, the author proceeded to apply my new method, and my new disturbing function (H_2), to my new conception of the elliptic *orbits* of the planets, which is entirely distinct from that which was so beautifully imagined by Lagrange; and that he has, with great elegance and, so far as I could judge, with success—though on so important a point in science I must reserve myself for a more careful judgment—applied my principles to the Perturbations of Jupiter, produced by all the other planets; and has gone on to calculate the elements for the years of our era, 2300, and 2800. Mr. Grant could not know of M. Hoüel's book, since it was published later than his own; but what an *enormous* difference of scientific sagacity in appreciating discovery the works convey! A certain "De Morgan," to whom he refers, paid me all justice.'

[The next letter from Hamilton was a long one in two instalments, both dated Nov. 26, 1856, on the subject of Decimal Coinage. I extract a few passages from De Morgan's reply]:—

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 26, 1856.

'I got your nine-sheet letters a day or two after I saw your son, who called on me in an evening on which my wife was laid up with bronchitis, now passing away. When the house is free from this plague, I hope to see more of him. I think it is nine or ten months since I heard from you, or *vice versâ*. I concluded you reasonably busy. Your namesake has passed away since I last wrote, and I, of all persons, wrote the notice of him in the *Athenæum*.

'When you give me express permission, I shall be very glad to put your name down on the Decimal Association. No subscrip-

tion is required. . . . The creed of the Association is, "There is no pound but the pound, and the mil is its thousandth part." All questions of nomenclature are open. The decimalised pound is the bond of union. . . . With the children [Hamilton was proposing to lecture the children at Mercer's School on the decimal system] you may do anything; with the grown people the difficulty is to get them to suppose it possible that they can put their heads to anything in the shape of calculation. The only plan is to try to get the extra farthing in sixpence into their heads, and then cheat them into what you can of the rest. But they will never believe they have actually done it, if you allow them to think there is anything to do. . . .

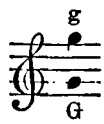
'I heard from your son that you were all pretty well. Steer clear of bronchitis. There is no saying what the isonosical lines are. The cholera used to traverse India in three strips. One of them passed through a regiment encamped, in the night, decimated the centre companies, and left both flanks quite untouched.'

From A. De Morgan *to* Sir W. R. Hamilton.

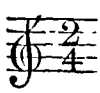
'7, Camden-street, Camden Town,
'*January* 12, 1857.

'I send you the air to the Gentleman, which is common to the English Gent., the Irish Gent., and all the gent. songs. I send it in my own musical notation. It is a chant:—

AA minim,
A_0 crotchet,
$A_,$ quaver,
$A_{,,}$ semi-quaver, &c.



Key of G



$G_, \mid G_, G_, \ldots\ldots G_, G_, \mid A_{,,} G_{,,}$

$F_{,,} G_{,,} \mid A_{0,} A_, \mid A_, A_, \ldots\ldots A_, A_, \mid$

$B_{,,} A_{,,} G_{,,} A_{,,} \mid B_{0,} B_, \mid B_, B_, \ldots\ldots$

$B_, B_, \mid C_{,,} B_{,,} A_{,,} B_{,,} \mid C_{0,} C_, \mid C_, C_,$

$\ldots\ldots C_, C_, \mid D_{,,} C_{,,} B_{,,} C_{,,} \mid D_{0,,}$

$G_{,,} \mid G_, g_, F_{,,} E_{,,} \mid E_, D_, G_{,,} A_{,,} \mid$

BB | AA | GG | GG‖

'I made out a system of printing music by which a common printer could manage to give it, when an author is hard pressed, and does not like to go to the expense of engraving. And I had set up very satisfactorily the opening movement of the overture to Prometheus—treble and bass.' *

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*January* 18, 1857.

'Your theorem [?] is under consideration. I dare say it is true; and it will set you going in a new groove.

'T'other Sir W. H. said that there was no more merit in being right in mathematics than in walking straight in a ditch. He forgot the two questions:—1. Given two points, required to make a ditch between them. 2. Given a point where *n* ditches meet, required the way of knowing the right one. All metaphysicians—except you and I, and Boole—consider mathematics as four books of Euclid, and algebra up to quadratick equations.'

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* *From* GEORGE PETRIE, LL.D., *to* SIR W. R. HAMILTON.

'*February* 18, 1857.

'I found no difficulty in reading at sight Mr. De M.'s notation of the tune of the Irish Gentleman, which, by-the-way, delighted me by its ingenuity. An ignoramus in mathematics like myself would never think of such a clear yet simple mode of conveying musical sounds to the mind as this. And with respect to the tune itself, I wish you would acquaint Mr. De M. that I consider it as in no respect Irish. It is in fact a very pleasing English melody, founded upon the style of the cathedral chants, and expressly composed for the song of the Old English Gentleman, of which the Old Irish Gent. is no doubt a sort of imitation or parody. This latter gent., by-the-way, I have an indistinct impression of having read in our *University Magazine*, or some other and perhaps older Irish periodical. I have certainly read it somewhere in my time, but it was long, long ago. But I never heard it sung.' De Morgan subsequently writes:—'I am glad Dr. Petrie likes the musical notation. Did I send you the printed specimen in *Notes and Queries*? I will procure one if I did not. I never thought the air was *Irish*: it is clearly an English cathedral chant. I cannot yet find the words anywhere, or hear of anyone who has heard of them. The song is an imitation of the Old English Gentleman, as now sung, which itself is an imitation of the old version, which may be seen in the "Elegant Extracts."' Of the words of the Irish Gentleman Hamilton writes:—'My daughter thought your own addition the only pleasing part of it. . . . . I don't like it *at all* . . . I *don't* like the verses, but I have not destroyed anything from YOU.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *March* 22, 1857.

'I have a Godsend for you—that is if it be new to *you*, which must be owned to be unlikely—namely, a title-page.

'While my daughter and I went to our parish-church of Castleknock . . . my second son, Archibald, . . . walked to the other Protestant church in our neighbourhood, namely, that of Finglas, where the poet, Parnell, if I mistake not, once was Vicar—a fact which I think that I have heard mentioned, long ago, in some charity sermons *there!* without admiring the taste of the introduction of such an anecdote in such a connexion—unless, indeed, the charity should in any way date from him, a point into which it may be worth while to inquire. Archy is a particular pet of the present Finglas Vicar, Mr. O'Regan, who succeeded to Dr. Walsh, once Chaplain to Lord Strangford in the Embassy at Constantinople, and otherwise known as a traveller and as an author. Dr. Walsh and I were great friends: one of my earliest recollections is of my father's having taken for the summer a house of his at Glasnevin, in this neighbourhood, and of my pursuing there some daring and forbidden explorations; mounting on tables and chairs, to reach a sort of angle in the roof, whence I contrived to drop into a locked-up lumber-room, where I only found some old dials and things of no importance. But the chamber had the charm of prohibition, having probably been reserved by Dr. Walsh, before he went to the East, or, at all events, before he set the house, which still exists, to my father. Be that as it may, Dr. Walsh quite forgave me, and laughed at the story when I told it to him, after (say) twenty or twenty-five years; but at the time I must have been embarassed to *emerge*. (Facilis descensus . . . sed revocare gradum . . .). Some ladder I think was put down for me, and I got, of course, a very good scolding, which does not prevent me from remembering that Glasnevin house with a certain affection and interest. Child as I was, I well recall my reading Shakespeare in bed, on some glorious summer morning, perhaps at five o'clock, while with a half-averted glance I watched, from time to time, the gambols of two kittens, of which one occupied in triumph the seat of a chair by my bedside, while the other attempted to dispossess

her. Forty years have passed away, for I speak of 1817; but I feel that I make no misnomer, when I say the names of the kittens were Molly and Jane. With the true spirit of a boy come back upon me, I name my *own* pet, Molly, the first; the other, Jane, was the pet of my sister, Eliza, the poetess. We used to let them out, sometimes separately, at two different holes in the roof, and then run out on the leads, to enjoy their surprise at meeting each other. It is, no doubt, a consolation to me, that the love between my sister and myself was *never* interrupted; and that, at last, she died in my arms.

'But to return to the present Vicar of Finglas, Mr. O'Regan, and my son Arch. The latter has brought to me from the former that new old book which has set me upon writing this letter. The title-page runs thus:—BERNHARDI VARENJ Med. D. GEOGRAPHIA GENERALIS, In quâ Affectiones Generales Telluris EXPLICANTUR, summâ curâ quam plurimis in locis Emendata, et XXXIII. Schematibus Novis Ære incisis, unà cum Tabb. aliquot quæ desiderabantur Aucta et Illustrata Ab ISAACO NEWTON Math. Prof. Lucasiano apud Cantabrigienses. Editio Secunda Auctior et Emendatior. Cantabrigiæ. Ex Officina Joann. Hayes, Celeberrimæ Academiæ Typographi, sumptibus Henrici Dickinson Bibliopolæ, MDCLXXXI. So ends the title-page.'

[This letter enclosed a note from Mrs. T. Disney, mentioning her remembrance of Prof. De Morgan's brother, and other members of his family, and also giving an account of Hamilton's voting for her friend the Rev. Dominic Browne, candidate for the Secretaryship of the Association for Promoting Christian Knowledge. Hamilton's perusal of this note led to his making the following comment upon it in a postscript to his own letter. I have felt obliged to omit introductory reminiscences, most of which have been elsewhere recorded.]

'P. S.—After finding that I should be too late to catch the Dublin post of this evening, if I had sent off the enclosed letter by a messenger, I am induced to add the remark which may interest you in connexion with Historical Evidence, and the Calculus of Probabilities, that a statement in the lady's note within is *not* historically accurate—although she had it from what might

seem the *best* authority, and no motive could even be *conceived* to have existed to vitiate the correctness of the narrative. . . . At all events, I had a right to vote in a recent and contested election for a vacant Secretaryship, combined with a Sub-Treasurership, in which, though only a layman myself, I stood, as a member, on an equal footing with that fine old gentleman—that REAL "Old Irish Gentleman," the Lord Primate of Ireland, Beresford [Lord John George]. The Primate had been induced to write a good-natured letter to a son of an old chaplain of his own, commencing with the words, "My dear James"; and on the strength of those *words*, a large body of clergymen, revering justly the Archbishop of Armagh, were induced to vote for a young Mr. ——. The Archbishop of Dublin remained neutral. I voted against "Dear James," and avowed openly that I was balloting for the Mr. Browne who succeeded. But it is NOT TRUE that I "*sat down*"! In fact I do not think that there was any chair or other seat in the balloting-room except the small bench on which the acting secretaries sat. So much for historical accuracy on even recent and public occurrences. But the substance of the account is quite correct. Draw your own moral, and believe me,' &c.

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 4, 1857.

'. . . Newton's Varenius I am well acquainted with. I once examined it narrowly to know what made *him* edit it. I fancy it was the full description and defence of Copernicanism which made him desirous of introducing it into the University, in which at that time the Copernican system was far from fully established. But Newton was not much of an editor. He allowed Varenius's mistakes about the English mile to remain without comment.

'You climbed up to the hole in the wall which was patent; but I discovered the hole. You shall see. I was about twelve years old, and well up in the Castle of Otranto, and lots of other Gothic stories, and not wholly incredulous, when a Somersetshire rector lent my mother his house for five or six weeks. It had been attached to an old priory, the moat of which, bricked into a Dutch kind of canal, watered the garden; and round this moat would often drive, as we were told, carriages and horses, and passengers

and coachmen, all without heads, at least all but the carriages. So I looked about the house, which was very old, and in some parts gloomy, and I found in what novelists would call a gallery, but which was a passage leading to bedrooms, a quantity of paneling. One of the panels seemed a little loose, and I worked at it, until at last up I made it slide, showing a passage big enough for me to crawl into. Ah! thinks I, it's all true; there *are* secret passages; if so, why—there must be the skeletons and the bloody armour, and the deep groans from underground. After a great deal of hesitation I determined to crawl in, and I did it, and emerged in a room of which the beams were bare and the aspect most ghostly, except for spiders and webs. But not a skeleton nor a single suit of armour. The room had in it a door which opened on a stair-case as dark as the passage; and here I trembled again, and at last went down. A door at the bottom opened and destroyed the romance, for I found myself in the old oak-paneled drawingroom. But why the best bedroom—as it ought to have been—was devoted to spiders I never learnt. A haunted room, perhaps. The passage, no doubt, was for concealed exit and entry, and I dare say parsons in Cromwell's time had found it useful. So no more at present, this being story for story, from yours very truly.'

'*May* 15.—The instance of historical evidence duly received. What is history? Might it not as well have been anything else for aught we can know? Did William III. really come over to keep us from wooden shoes? You observe that it is now contested that Shakspere wrote his own plays; somebody says Bacon wrote them. Are we quite sure it was not Dean Swift who deposed James II. and reigned in his stead? if indeed James was deposed? or if he ever reigned? supposing him to have existed. Or do we exist ourselves? Or does anything exist? Is it not altogether a prejudice of our own for our own glorification? I wonder nobody has started a theory of universal nihility—showing *à priori*, *à posteriori*, *à fortiori*, and *ab ex nihilo nihil fitiŏri*—that nothing ever did or can exist, or will exist, or could be supposed to exist. Whately says you must always look for the weak point of a system in what is universally granted; now do we not all grant existence an existence? Has anybody proved the existence of existence? Could he do it without assuming the existence of the existence of existence, and so on *ad infinitum*? These are grave questions. The Sir W. H.

to whom they belong is gone; therefore they are properly relegated —to use a word of his—to his namesake.

'The French Government are beginning to do justice to Libri. They have restored his books and effects, and have designated him in the proceeding as *absent légalement* instead of *contumace*.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 20, 1857.

'It was a curious piece of good luck of my son to be a lodger next door to you, for a while, without having designed to be so. It is clear that Mr. Salmon thinks the success (such as it is) of the quaternions to be a similar piece of good luck—or at least that they form an intransferable sort of Bank Post Bill, useful only to the proper possessor of it. I have recently made out an interesting property of the envelope of the plane connecting two corresponding sides of a Hessian or other Cubic Cone—by quaternions; but Salmon would call my method a purely *spontaneous* one! Return to me his note, and believe me,' &c.

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*June* 15, 1857.

'I return Mr. Salmon's letter. His very name is redolent of recondite curve properties. What's in a name? If Lobster should ever write on the higher curves, or, better still, if Salad should, and Lobster should publish an enlarged edition, then Lobster's Salad will suggest we don't know what ideas of symbolised space.

'On with you. You have the whip hand of the quaternions, which hardly anybody else will get in your time.

'I am very sorry that we saw so little of your son. He is an Irishman—loves fun—and sticks to business—three capital things to come together. I have known the first and second come together often. I *have* known the third absent.

'I have arrived at the necessity of enlarging the definition of *affirmative* and *negative*, as applied to propositions. No name filling the universe, which is an essential condition of names used in predication, we have *identical* names and *contrary* names, which

together fill the universe, but have nothing in common. An *affirmative* proposition is anyone which is necessarily *true* of *identical* names, *false* of *contrary* names. A negative proposition is any which is necessarily *true* of *contrary* names, *false* of *identical* names. To this complexion must it come at last.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *June* 24, 1857.

'You have made my face white, by sending me those lines for Dr. Petrie [a copy by De Morgan of The Old Irish Gentleman], into whose hands I duly gave them, on Monday night, at our last meeting for the session of the Royal Irish Academy. Mind, *not* "Hibernian," though the *Times*, in one of its flings against Ireland, professed to see no difference.

'I suppose you are not likely to have heard, or to remember, that I had once (long ago) the honour of a sort of quarrel with the *Times*. At all events, during its anti-British-Association, or pre-scientific days, probably while Sir James South was king, I was led on, by the ardour of some speech of mine at Cork, to *ridicule* (in presence of Lord Rosse, &c.) the Thunderer's assumption of being any sort of judge whether the B. A. was right or not. I must say, for the London Jupiter, that he took it very good-humouredly, and only playfully inquired, *how much wiser* Sir W. R. H. supposed the boots and ostlers of Cork would be for his profound mathematics—or some such joke—which of course I took in good part.

'People talk of *Irish impudence;* but for powers of *face*, commend me to the London *Times!* I can remember the appearance of an article, or an adopted letter, in that newspaper, in the winter of 1834, when Wm. IV. turned out his ministers, the last passage of which article or letter was this: "The Queen (= Adelaide) has done it all!" And then scarcely a day (I am pretty sure that not a whole week) elapsed, before the *Times* became an eloquent advocate for that singularly *consistent* Cabinet, of which the Duke of Wellington, the Duke of Vittoria, the Marquis of Douro, the Count of Salamanca, &c., &c., composed at first the only individuals. (I beg *their* pardon if I forget all their *names*.) It has done me good to write some nonsense, and I am,' &c.

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*July* 3, 1857.

'I suspect you write letters as hens lay eggs, and that Lady Hamilton finds them, envelopes them, puts them before you as official letters, and you direct them as per memorandum affixed.

'I see now what use you, philomath, make of your position as *astronomer*. In a book printed by some lieutenant, showing that the earth moves trochoidally if the sun has a motion, the author says that he submitted his diagram to you, and that you told him that you could only give him ten minutes, for that you *had an observation* coming on in a quarter of an hour. I see now how you get rid of visitors whom you do not want. This is clever.

'And this is innocent. You say that people talk of Irish impudence, but you will back the London *Times* against them. Why, does not all the world know that the bulk of the *Times* staff is Irish? Some people say they make a rule always to have one editor Irish, to keep them from *not* going too far. Activity in negation is not an English idiom, generally. It may be *Oriental*. A long time ago a child, a cousin from India, whose nurse-tongue was Telugu, and who could hardly put together phrases in English, told me my hooked stick was "to not let the dogs run." I found out by cross-questioning that this was his idea of the intention of it. [An illustration follows.]

'The course you describe is the regular one of the newspapers, only the *Times* does it with a bolder face. They live from day to day, and it is enough if they can be with the tide of the day. I have long made up my mind that newspapers are only attempts to reflect the features of opinion for the time being. No newspaper, which means to exist, can be much more or much less absurd, dishonest or prevaricating, than the public it is to sell to.

'And people who call newspapers names are people who quarrel with the mirror for showing them a set of ugly features.'

[A letter of Hamilton's, referred to in the following, is missing].

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*August* 3, 1857.

'You did not send your Letter to a Lady, but I saw it in the *Cyclopædia*. I suppose Lady Hamilton has no objection to your writing to a lady about quaternions, as long as you do not extract the square root, and write on *binions*. But really, now, I wonder at your supposing that you could make quaternions go on all fours as a ladies' pad. They do not canter gently enough.

'Now to the Bill, which shall be confidential. . . .

'This remedied, I like your draft very well in all its matter.

'Archibald Smith was first introduced to me at an evening party as "Mr. Smith," and I had to identify him by his conversation, which was about transformation of multiple integrals, by which *differentia* I reduced the *genus* Smith to the individual Archibald. [Now Mr. Justice A. L. Smith.]

'As to letters, I keep a very large number, but I have a prudent executor in my will, who will deal properly with them. I do not think you need mind any of your letters being published. You say there ought to be limits to human folly. I am not sure of that. At any rate, if all my letters were published, I should have no limits whatever. Why? what did I send to the author of the *Prolegomena Logica* the other day, merely because he is a grave man, is Mansel, and I hoped to raise his dander, as the Yankees say—nothing less than a drawing of Hood's ambiguous middle term—

"So they told the sexton,
And the sexton tolled the bell,"

which I illustrated at length by something like this, only more elaborate. [Illustration follows.]

'Now with regard to wills . . . Beware of codicils. A man's intentions die with him, and his words only remain, and his intentions are gathered from them by *rules*, which he does not know, probably.

'A cousin of mine—Irish by-the-way—the late Major Darley of His Majesty's tenth regiment, being in India, and meaning to leave his orderly-sergeant all his furniture and traps, left him "all

his 'goods and chattels' *in India.*" Now one of his Indian *chattels* —but he did not know the meaning of the word—was £10,000 in Company's paper, and it was only redeemed by a vexatious law-suit. He had better have left all his *traps*, and then he would not have left such a trap. So beware of legal words. If a lawyer draws the will, it is well to have them. But the best thing a layman can do is to write his will just as he would give full instructions to a solicitor.

'Don't be intestate for a single day. Make a rough draft, and execute it, to save the few days of making a better one. I take it for granted you know the essentials of witnessing, &c.

'I have been at the logic, and have commenced a new warfare with the logicians. They really are not fit to handle their own tools without mathematics. They have turned all logic into mathematics—without knowing it. And now that they begin to see that logic has two sides, which ought to be called the mathematical and the metaphysical, but which they call *extension* and *comprehension*, they are only growing another branch of the mathematical. They want a mathematician to show them the way to the door.

'You sent me (Feb. 1856) a pretty reduction *ad impossibile*, &c., derived from $A + B + C = 0$. This is an instance of a wrong analogy having its right points. I have inserted it in my Paper.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 7, 1857.

'It may *amuse* you—and certainly I do not aspire to *instruct* you —if I mention that I have this day dashed off a note (about the thirtieth of perfectly pleasant ones, since there was something like a quarrel between us, or rather between our *friends*—you know what *that* means—Salmon* and I have never had one *moment's* unkindness or jealousy towards *each other*), which note began nearly as follows:—"My dear Salmon, a brilliant thought has occurred to me on which I should like to have your opinion."

'It was a project of a mathematical "Tract," on which I consulted, or *non*-consulted him; for I told him plainly, that whether he said Yes or No, I would, if in the vein, go on.

* Even with poor (but great) Mac Cullagh I was *always* on *good*, and I might add, almost always on *affectionate* terms.

'But I said, with equal honesty, that his opinion on the point would *interest* me very much.

'In the same spirit I write to *you*, De Morgan, to mention that it has just occurred to me that a "Tract," of perhaps fifty pages, or suppose that it stretched to one hundred, might be drawn up by me, and be entitled, "On the treatment, by Quaternions, of Cones of the Second Order" (you know, from Dr. Hart's note to me, that I have lately found out *something* about Cones of the *Third* Order): and that such a Tract, *if not allowed to become too long*, might fairly be made a *part of undergraduate study* among young men seeking for *honours* in Science. In *Dublin* it would have a peculiar appropriateness, because Cones and Conics have been very generally studied by clever young men here; and a translation of some of the Memoirs of Chasles, by C. Graves, has been circulated, and examined in; with *additions* of Graves's own, which I ought to study more closely than I have yet done.

'Now, dear De Morgan, I am not asking you to *advise* me as a *friend*, but merely to tell me how you *relish* (or *dis*relish) the project, *as a scientific man;* and as one well acquainted with University life and studies in England.

'It is mere matter of fact that I possess a large amount of *materials;* and I should hope that, in point of *authorship*, a *second book* of mine might have a chance of being *better* than the first; which is not asserting a great deal.

'Thanks for your long letter.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*August* 14, 1857.

'I answer your question first as to science in general; next, as to University studies.

'1. A tract on quaternions, brief, of syllabus character, and not lengthy in the elementary parts, would be a very desirable thing. It must not be on cones alone, but may take that or any other application to fill out upon.

'2. For the Cambridge undergraduate such a thing is nugatory. The system there, for the higher men, has degenerated into examination work under private tutors. An extra subject, such as quaternions, would be quite neglected. And the Course is so

loaded already, that nothing but the pressure of examiners would induce the men to take a new subject.

'Further, the Cambridge tendency is towards physical application. A great part of what so many of you have made elementary to the higher Dublin men is Sanscrit to the high Wrangler as yet.

'If a man were determined to publish, and could be restrained from making too large a tract, keeping only the *very fundamental* in view, he would put a temptation in the way of a few of the highest men, which would in time produce a determination on the part of some one of these men—when in office—to *examine* in quaternions. The thing would then be done. As to any sale at Cambridge, *now*, doing anything perceptible towards paying the expenses, it is beyond all reasonable probability.

'The greater part of the independent readers would, I verily believe, be found in Oxford. What number that would amount to I have not the least idea.

'But Dublin would furnish the largest contingent.

'And now you have all I can say. I am afraid the book would be a loss of £ *s. d.*; in every other point of view it is an excellent idea.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 14, 1857.

'You know me too long to be surprised by any oddity of manner of mine; yet I think that I shall surprise you by an oddity of conduct; I have actually taken, and acted on, *advice!*

'At least, I have abandoned a project, which for a day or two had been a pet one, of writing something about quaternions, for our "Moderatorship" men in T. C. D. Salmon tells me that they have too much to do as it is, and I believe him.

'But Salmon thinks that the interval between the "Moderatorship," or the "Degree" Examination here, and the Examination for Bishop Law's "Mathematical Prize," might very properly be devoted, in part, to a study of the quaternions.

'Consequently, I do not abandon the hope of writing *something elementary*, by which I mean something about Sphero-Conics, and Surfaces of the second order, with a little touch at Cubo-Conics, or the intersection of a sphere with cones of the third order.

'A *short* pamphlet, or tract, upon such subjects, might not

perhaps cost much in money; but really my Alma Mater has behaved so handsomely to me about my Book, that I am not inclined to ask for any new book-grant.

'For the *immediate* object of preparing Bishop Law's prize-men in Dublin, my own lectures and luncheons do almost enough (I lately gave a course of oral lectures); but as regards the training up of a new race of "quaternionists," I think that some short "tract," or pamphlet of my own might be important.

'*August* 17.—This note has been lying by me for some days, and yours has reached me in the interval. Your remarks appear to be very just, and at all events that whim of mine has passed away for the present. I am not at all impatient for early attention to the quaternions; but shall perhaps go on to collect *examples* of work in that calculus, so as to have them *ready* for the time, which (according to my expectations or hopes) is sure to *come*, when a certain *class* of students will desire to possess them. Meanwhile I have written a whole lot of letters to Salmon on the subject, and the enclosed note (which I wish you to return) will show that he has already so far *learned* the principles of the quaternion calculus as to be able to work *for himself*. And in several other instances in Dublin I have found my methods to be *communicable*.

'P.S.—We are very busy in Dublin about the approaching visit of the British Association; but I believe that *you* really despise the whole thing as much as the *Times* used to *affect* to do.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*August* 21, 1857.

'I do not *despise* the British Association. It is useful in its way, and is pleasant to people who like travelling. There used to be ferocious interlaudation, but I think that habit has subsided a little. If I have never been at one of their meetings, I have also never been at a meeting of the Royal Society. Never once in my life. I have, in fact, no ideas or sympathies in common with the *physical philosopher*. He must be more of a psychologist before I can tolerate him. The astronomer and I get along well enough. We start together at $\frac{m}{r^2}$. I found out that the *physician* and I

were two, long before I knew why. The British Association promised us all kinds of Reports upon the history of science; but they have not done much. It is easier to collect people to hear and tell some new thing than to set them quietly at work over old books.

'However, it does good—makes people acquainted with science in places which might become torpid—which is politic, though the method is rather Polito-ic,* if you understand such a pun. But the great objection with me is the *travelling*, which I never do except under very high pressure. I consider I had my share of it in my nurse's arms, in which I began life with a journey of 11,000 miles, crossed the line twice, and knew nothing about it all—Heaven be praised.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,

'*November* 15, 1857.

'Your tract† received, and many thanks, and I will read same. Do you know the following? If not, I suspect any proof but one will be rather complicated. Euclid says that two straight lines cannot inclose a space. What are the classes of curves which, by the nature of things, cannot do this? One answer is this:—Let

$$f(x+y\sqrt{-1}) = \phi(x,y) + \psi(x,y)\,.\,\sqrt{-1}$$

be a function for which $\phi(x, y)$ and $\psi(x, y)$ never become infinite, either of them, for any finite values of x and y. Then

$$\phi(x,y) = 0, \quad \psi(x,y) = 0$$

are two such curves.

'Perhaps they do not include all such curves:

$$fz = (p+q\sqrt{-1})\,z + r + s\sqrt{-1}$$

only gives *perpendicular* straight lines.

'I shall send you, to-morrow, my answers to Lord Overstone's questions.

* Referring to Polito, the menagerie-keeper.

† Probably Sir W. R. H.'s Paper in the *Philosophical Magazine* of November, 1857, on Multiple and Definite Integrals.

'I have got only an odd scrap of paper, but I do not know how it is to be filled, having no news of any kind. I don't want discount, and so am personally unconcerned in the bank question. I have lost no friends in any mysterious way, and so have no direct interest in the carpet bag.* My share in the horrors of India is limited to two young and remote cousins whom I never saw. So public affairs are but public with me. I hope they are the same with you.'

[On the 16th November Hamilton enters on a fytte of this correspondence which passes far on into 1858. It was suggested by his communication to De Morgan of his Paper on Multiple and Definite Integrals, published in the November number of the *Philosophical Magazine*. The results of that Paper are in these letters largely extended. I shall here set down, in Hamilton's words, what appear to be the leading propositions, and I shall add extracts from incidental discussions by both correspondents on convergent and divergent series, on the proof that every equation has a root, on the transition from double algebra to quaternions, and a few other matters mathematical and non-mathematical.]

'*December* 4, 1857.—Theorem respecting Definite Integrals, and Transformation of Series:—

'Theorem (A):—"If we have the two developments, ascending and descending,

$$\psi x = a_0 - a_1 x + a_2 x^2 - \&c., \qquad (1)$$

$$\psi x = b_1 x^{-1} - b_2 x^{-2} + b_3 x^{-3} - \&c., \qquad (2)$$

for any one function ψx algebraical or transcendental; and if the following third series,

$$ft = a_0 - a_1 t + \frac{a_2 t^2}{1.2} - \frac{a_3 t^3}{1.2.3} + \&c., \qquad (3)$$

can be summed so as to give a function of t which is real, finite, and continuous, with its differential co-efficients of all orders from $t = 0$ to $t = \infty$, and which vanishes, with all those co-efficients, at this last limit, $t = \infty$: then

$$b_n = \int_0^\infty dt \left(\int_t^\infty dt \right)^{n-1} ft." \qquad (A) \quad (4)$$

* An allusion to a mysterious murder of that day.

THEOREM (B):—"If there be a function ft, satisfying the conditions of Theorem (A), and developable, as in that theorem, in a series of the form (3); and if after deducing from it the connected series (1) for another function ψx, we can develop the latter function in a series of the form (2); if also we write, for abridgment

$$f_n t = \frac{b_1 t^{n-1}}{1.2.3\ldots(n-1)} - \frac{b_2 t^{n-2}}{1.2.3\ldots(n-2)} + \&c., \qquad (3)'$$

with exclusion of all negative powers of t: then

$$\left(\int_0^t dt\right) ft - \left(-\int_t^\infty dt\right)^n ft = f_n t."\ *$$

'*December* 22, 1857.—THEOREM (C):—"If ft and $\chi t \,.\, ft$ (although not necessarily the factor χt itself) be functions of t, which remain finite, real, and continuous, while t varies from 0 to ∞, but which vanish with their differential co-efficients of all (positive) orders at the limit $t = \infty$; then

$$\chi\left(x\frac{d}{dx}\,x\right) K(x+J)^{-1} ft = K(x+J)^{-1}(\chi t \,.\, ft);$$

where $(x+J)^{-1}$ is interpreted as $x^{-1} - x^{-2}J + x^{-3}J^2 - \&c.$; and J, K denote, respectively, the two symbols of operation,

$$J = \int_t^\infty dt,\ K = \int_0^\infty dt."$$

'*December* 23, 1857. . . . 'Hence it appears to me not too bold to enunciate the following theorem:—

* In the list of the Papers to be read on December, 14, 1857, contained in the notice-paper of the Royal Irish Academy, the third item is—'Sir W. R. H., LL.D., "On two general Theorems respecting an extensive class of Definite Integrals."' These Theorems were the above Theorems (A) and (B). By a letter of this Correspondence, from Hamilton, dated December 15, it appears that he failed to be present at the Meeting of the 14th. He adds, 'However, Charles Graves [then Secretary of the R. I. A.] had "saved date" for me by reading an extract from a letter to himself.' The *Proceedings* of the R. I. A. contain no record of this communication. They record that he had given an account of his researches on the subject on the 9th of November, 1857, and that subsequently, on January 11, 1858, he read a Paper 'on some General Theorems in the Calculus of Definite Integrals,' but it appears that the Paper was not printed.

'THEOREM (D):—"If ft be a function of the real variable t, which remains finite, real, and continuous, with all its differential co-efficients of positive orders, while t varies between two given real limits a and b (where b may be supposed $> a$), and which vanishes with all those differential co-efficients at the latter limit b; if also we find that the ascending series,

$$\psi x = fa + (x-a)f'a + (x-a)^2 f''a + \&c. \qquad (1)$$

can be definitely transformed into a descending series of the form

$$\psi x = b_1 (x-a)^{-1} - b_2 (x-a)^{-2} + b_3 (x-a)^{-3} - \&c., \qquad (2)$$

by recognised principles of analysis, we may then expect to find that the co-efficient b_n is equal to the following definite integral:

$$b_n = \int_a^b dt \left(\int_t^b dt \right)^{n-1} ft = \lim_{t=a} \left(\int_t^b dt \right)^n ft." \qquad (3)$$

'I can assign, in like manner, a Theorem (E), which shall be an analogous extension of my former Theorem (B). You received, I hope, a copy of Theorem (C), which *also* I may be able to extend to the case of two *arbitrary limits of integration*.'

'*January* 8, 1858.—I want to tell you of a Theorem (E), not perhaps *perfectly* proved as yet, but of the correctness of which I feel pretty confident; and which substantially includes my former theorems, and others.

'THEOREM (E).—"Let a, b, and a_0, a_1, a_2, ... be constants, whereof only b shall be permitted to become infinite, positively or negatively. Suppose also that

$$fx = a_0 - a_1 (x-a) + a_2 \frac{(x-a)^2}{1.2} - a_3 \frac{(x-a)^3}{1.2.3} + \&c.$$

is a function such that it tends to 0, with all its derived functions,

$$f'x = -a_1 + a_2 (x-a) - a_3 \frac{(x-a)^2}{1.2} + \&c.,$$
$$f''x = a_2 - a_3 (x-a) + \&c.,$$
$$f'''x = -a_3 + \&c.,$$

when x being $\left\{ \begin{matrix} > a, \text{ but } < b, \\ \text{or } < a, \text{ but } > b, \end{matrix} \right\}$ tends to become $= b$.

Then the quotient

$$Fx = \frac{a_0 - a_1 \dfrac{(x-a)}{1.2} + a_2 \dfrac{(x-a)^3}{1.2.3} - \cdots}{a_0 - a_1 (x-a) + a_2 (x-a)^2 - \cdots}$$

tends to become = 1, when x tends as before to b."
Or, "If $fb = f'b = f''b = \cdots = 0$, then $Fb = 1$."

'I may shortly send examples. So far as I have *observed*, the numerator of Fx is a converging series, and the denominator is a diverging one; and *perhaps* this is *necessary*.'

[The letters from which the above theorems are extracted are very numerous, and contain a very large amount of mathematical work, culminating in two enormous letters devoted to the subject of Definite Integrals. The first of these letters bears as its initial date Feb. 15, 1858, and extends to seventy-two folio pages, the last page being dated May 22. The second letter is dated on the first page July 15, on the last page August 19.* It extends to twenty-four folio pages. The treatment of the subject contained in them is concluded in a letter of Hamilton's, bearing the two dates of December 8, 1858, and January 3, 1859.

I now proceed to give additional extracts from the correspondence of this period.]

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*November* 20, 1857.

'. . . I am reading your definite integrals, with the additional matter in your notes. Your remarks on the Cauchy and principal values remind me of a proof I have by me, but have not looked at for years, of the truth of Cauchy's principal value. Till I got that proof, I never admitted his right to pick out his principal value.

* It may be mentioned that the last four pages of this letter consist of a comparative view of Hamilton's results, and those published by Professor G. G. Stokes, in the Paper to which reference is made in Hamilton's letter of November 23.

'My proof of the curves which do not inclose space is so very simple that no one could miss it who came near the ground. It forms a pendent to a method I have lately found of proving that every equation has a root, which is so elementary that it must be *the* proof in future. I will send it when the Paper appears,* or before, if I have leisure for writing it out. It does not depend at all upon the differential calculus, or on anything but pure continuity.

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 23, 1857.

'I am very glad to hear that you think you have discovered *the* proof of the general existence of a root of an algebraical equation. Notwithstanding all that had been done by others, it was (for perfect repose to the mind) a thing still to be sought for. Perhaps Cauchy's proof came nearest to satisfying me, of any which I have tried to examine. But even Mourey's method, with some correction of details, was not (I thought) a total failure. You understand, therefore, that I am not asserting (or admitting) the insufficiency of all known proofs, when I say that I look forward with much interest to the communication of some *new* and *simple* proof by you.

'Still, as I am not *at* that subject just at present, it may be only honest, although it is a self-denying ordinance, to suggest to you not to waste (or spend) even half an hour of your truly valuable time in writing out an account of your method *for me*. Send it when printed (and if two copies, so much the better—perhaps I may have some payment *in kind* to make to you), and *then* I really expect to consider it, and hope to profit by it.

'But write, by all means, on *that* or any other subject, if you find yourself in a *mood* to do so. I sometimes send you written things, without at all expecting or much hoping that you will read

* It was published in the *Transactions* of the *Cambridge Philosophical Society*, vol. x., part i., pp. 261, and following. Read, December 7, 1857. Date at end, December 18, 1857. See, for condensed report, *Philosophical Magazine* for September, 1858, pp. 232-3. Professor Airy, following up this Paper of Professor De Morgan, contributed to a subsequent number of the *Cambridge Philosophical Transactions* a proof of his own that every equation has a root.

them soon, and perhaps chiefly for the purpose of clearing my *own* thoughts, although partly for the pleasure of corresponding. *Your* thoughts, however, are *always* so clear, to your own mind at least (though the subject may cause delay in other people finding them to be so), that you can never require to *assist yourself* in that way, which to me is occasionally useful.

'Professor Stokes has recently sent me, at my request conveyed through Dr Robinson (whose daughter Stokes has married), a second copy of his Paper (printed in the *Cambridge Philosophical Transactions* for 1850), "on the Numerical Calculation of a Class of Definite Integrals and Infinite Series"; the copy which he sent me some years ago having been mislaid, and so merely *glanced* at, when received, that I just *remembered* the author's referring, in a note, to my own Essay on Fluctuating Functions. I regret that I did not receive the duplicate in time to cite Stokes's Paper in my own little communication, for I could not venture to do so from memory; but there will be other opportunities. However, I only observe an *affinity*, and not as yet any *anticipation*.

'In my equation (26) [of Paper in *Philosophical Magazine* of November, on Definite Integrals], I wish you to insert with a pen, in line 2 of page 7 of your private copy, under sin 86° 49′ 52″, the factor 2 before $\sqrt{20\pi}$. You will find that its omission was a mere *mistranscription* (I scarcely venture to call it a *misprint*, so accurate are they in Taylor's printing office), and that it has not at all affected the arithmetical results. Any remarks of yours on my Paper will be interesting and instructive to me.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*November* 27, 1857.

'I am entirely of your mind. Sitting down to write with a purpose, whether a letter to a friend or a tract for one's pupils, is a process which clears the head of confusion, and often puts better things into it. I never knew till to-day—or if it be a common thing I always passed it over—that *every* convex spherical polygon has its *supplemental* polygon, so that all the angles of either are supplements of the sides of the other. The triangle is common, but the general case, though obvious enough, is never mentioned.

'Your mention of Mourey's proof—which I had forgotten the existence of—made me look at it again, and I find no affinity with mine, which is of the Cauchy family, and would have struck Cauchy, if he had lived, I suppose. Unless indeed, which may be true, it is to be taken that if a man does not see a very fundamental point in his own subject before he is well stiffened in it, he never sees it. My proof is so easy that there is no trouble in writing it out. Curiously enough, it proves that every equation has a root *at least*, and leaves it to the applicant to cut down the number by Cauchy's theorem . . .

[Same date, later.]—'I have been looking at Mourey's proof to-day, *i. e.* of the existence of the root of an equation. It is perfectly sound, and is, I feel sure, *the* double algebra proof. But it wants a little *intelligiblization* to suit it to modern notions. I never did more than glance at it; but I now see I wanted, what I have been doing lately, to get it *glanced* into me. I shall get it into my Paper on the subject.

'I have never been able to get Argand, who you say was the lamp of the subject. The number of independent arrivers at part or whole of double algebra—the tacking on of the exponents excepted, which I cannot find myself anticipated in—is very large. Airy told me twenty-five years ago that he had made $\sqrt{-1}$ stand upright for himself when an undergraduate.

'Herschel writes me—"What is the best preparation for, and resolution of, the discord of the compound sharp sixth?" To which he makes his own answer, *i. e.*—"The best preparation is to stop your ears before you strike it, and the best resolution is never to do such a thing again."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 28, 1857.

[After correcting some slips of the pen in a letter of Nov. 16, he proceeds:]—'I make no scruple of inserting pen and ink corrections in books which have been given to me, or which I have purchased: for instance, here and there in your *Differential and Integral Calculus*, although it is evident that you took great pains to avoid mistakes of the press. Was there *ever* a mathematical book printed which was absolutely *free* from such errors? I rarely turn over the pages of *any* book without my eye (which you know has

had long training in such things) detecting *some* misprint . . . I do not *remember* perceiving any misprint in your answers to Lord Overstone's questions [on the Decimal Coinage]: and as a general rule I admit that you are very careful and correct. Some authors are quite the contrary. Ivory complained to me that he could not read the first Paper of Lubbock's on physical astronomy in the *Philosophical Transactions* on account of the bad printing of the equations; and I fully sympathised with him.

'P. S.—I did not receive a single proof-sheet of my "Letters to a Lady," printed in Nichol's *Cyclopædia*, and therefore wash my hands of all *their* errors.'

'*December* 3.—It has been very pleasant and (I hope) useful to myself, to work a little at Definite Integrals lately; and even if the results to which I have arrived shall turn out to contain *nothing* new, although, I own, this seems to me unlikely, I am not going to sit down in a corner with my finger in my eye. . . . My results seem to point out some *new methods* for *transformation of series*, from ascending to descending powers, or *vice versa*, in connexion with definite integrals.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*December* 6, 1857.

'. . . Have you *got* Argand's tract (1806), or can you borrow it for me? Airy has lent me an old paper of his (MS.) written when he was a Freshman in 1820. He has $\sqrt{-1}$ $(\pm)^{\frac{1}{n}}$ well explained. But though he knew nothing of Argand or Buée (1806, 1815), he is more of 1815 than of 1806. There is an epidemic in the air about mathematical thoughts.'

'*December* 11 . . . I have been so busy this last week that I have not been able to do more than glance at your letter. All is new to me. But I have not been a reader of modern things for a long time. . . . Yours in all haste. (You wrote "yours in haste," therefore I am as much before you as the Archbishop of Canterbury is before the Archbishop of York. This does not look like haste; but a conceit is a legitimate stoppage anywhere and anyhow).'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 18, 1857.

'Stokes tells me that he thinks the integrals in my last Paper are all unlikely to have been anticipated, except *perhaps* the very easy one,

$$\int_0^\infty ft\,dt = \tfrac{1}{2},$$

which I despatched (as you may have seen) in a couple of lines. He has not yet been informed of my Theorems A and B. I am just posting to C. Graves my Fifth and Sixth *Examples*, each involving a group of results—in this instance easily verified. They shall soon be forwarded to you.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*December* 19, 1857.

'I have found time at last to read your last letters, which were a memoir in themselves. I have no doubt it is new. I have been before now smelling at the components of your theorem, by the calculus of operations, as a dog smells at something to see if it be good to eat; but I never got more than crude and uncooked forms. . . . This is the symbolical foundation of your theorem, but it is useless without the limitations which you seem to have found. . . . I have no doubt you have got hold of the right end of the thing.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 14, 1857.

'I warn you that this letter will relate, chiefly or wholly, to what seems to me a natural mode of transition from Argand's method to my own, or from Double to Quadruple Algebra: so that if you do not choose at present to read anything bearing on the Quaternions, you can lay the sheet aside. But if you should ever resume a half-formed intention which you once expressed to me, of at *some* time or other giving a few lectures on the last-named subject, I think that the following remarks might then be

not without utility. For my own part, when I attempt, as I sometimes do (not every year, but in the last spring, for example), to *teach* quaternions in Dublin, I regret not to find my hearers already *familiar* with Double Algebra, which seems to be little cultivated here. To be sure, I have thus a more open field to move in; more of a *tabula rasa*, whereon to inscribe my own speculations; but in point of historical fact and progress, I was *myself* familiar with Double Algebra, known to me at that time chiefly through Warren's book, before I thought of the quaternions; and both John Graves and I had for years been *seeking* for some *extension* of what we regarded as Warren's method, from lines in a plane to lines in space. And though I do not remember that *so simple a transition* ever occurred to me, during that search, as the one which I am about to mention, yet you will (I think) admit, if you read this letter, that it *might* have done so; and that a pupil of Argand or of Warren, or Mourey, or yourself, ought to find no difficulty in following it; but should rather feel surprise at its not having been thought of before, if such shall be found to be the fact.

'I regret to say that I have never seen a copy of Argand's publication of 1806, for which I would cheerfully pay more than for many a modern book; but John Graves, who is a great collector, has, in his fine library at Cheltenham, a set of Gergonne's *Annales de Mathématiques*, and he has assured me that he considers me not to have in the least degree overstated Argand's claims, in the Notes to the Preface to my Lectures. Indeed, through delicacy to a then living analyst, I said less on that point than I might have done; for I am pretty certain that it was from Argand that Cauchy took, without acknowledgment, the now universally received term "*modulus*" (module), of an imaginary expression, such as $a + \sqrt{-1}\, b$; at all events that term, with its present meaning, as $= \sqrt{(a^2 + b^2)}$, occurs in an early Paper by Argand in the *Annales* just now referred to, published (I think) not later than the year 1814, but at all events about that time; and if Cauchy had seen that Paper, it was scarcely candid of him not to mention it when he *adopted* the term in his *Cours d'Analyse* (Paris, 1821—by the way, I don't see the word "Paris" printed), page 183, with barely a "ce qu'on appelle," not likely to catch the notice of a reader. His passage is as follows, for you may not have the book at hand:—"Lorsque l'expression imaginaire $\alpha + \beta\sqrt{-1}$ se trouve

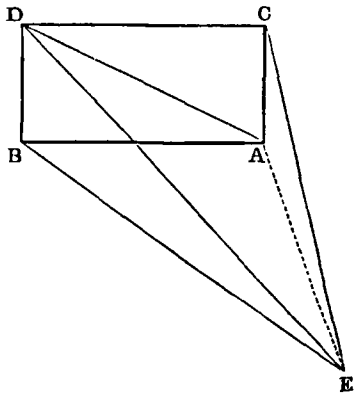
ramenée à la forme $\rho(\cos\theta + \sqrt{-1}\sin\theta)$, la quantité positive ρ est ce qu'on appelle le *module* de cette expression imaginaire; et ce qui reste après la suppression du module, c'est-à-dire, le facteur $\cos\theta + \sqrt{-1}\sin\theta$, est ce que nous nommerons *l'expression reduite.*" These answer, be it said, to the *tensor* and *versor* of a quaternion: but please to contrast the two phrases which I have underdotted—the "qu'on appelle" with the "que nous nommerons"—and tell me, or at least consider for yourself, whether beneath this difference of language there does not lurk a *half-confession* of an unstated anticipation.

'Without here inquiring to what extent Argand had himself been anticipated as regards the rule of *Addition of Lines*—although I believe him to have been the first who clearly saw and decisively expressed the rule that $AB + BC = AC$, and generally that $AB + BC + CD + \ldots KL = AL$—I take it that he is the first and undisputed discoverer of what he calls his *fundamental principle* (principe fondamental), respecting *Proportion of directed lines within one plane:* namely, that such proportion involves, and so far as directions are concerned depends upon, a certain *equality of angles*, attention being paid to the *sign* or *sense* of each angle, or to the direction in which the *rotation* is performed. Pupils of yours know well the value of these two principles, respecting *Addition* and *Proportion* of *Lines*, at least within one plane; and having indulged myself by renewing, as above, my little protest in favour of Argand, I have no objection now to speak of them as principles of *Double Algebra*, the extension of which *to space* is the thing next to be considered.

'In *double* algebra we assume an *unit of length*, and also *two rectangular directions*, as those of $+1$ and of $+\sqrt{-1}$; in the extended system sought, which it is natural to *suppose* will be a *triple* one, we cannot select *any one line* in space as having the *direction of positive unity;* because *no line* is *common to all planes*, even if these be drawn through one *common point*, or origin. We may, however, still assume a certain length as the unit, or standard of length; and a certain *direction of rotation*, right-handed or left-handed, as the *standard rotation*, by agreement or disagreement with which any other rotation, round any axis in space, shall be judged to be positive or negative. The simplest way of doing all this may be to assume three coinitial edges of *an unit-cube* or *three*

rectangular unit-lines from one common origin, such as *ijk* or *xyz*, with a *fixed order of succession*, and call the rotation round the 1st, from the 2nd to the 3rd, *positive*.

'From the want of a fixed line representing + 1, we cannot *immediately* construct, as in double algebra, the *product of two lines in space*. John Graves was once inclined to take, as a sort of *travelling unit-line*, the *intersection of the plane* of the two factors *with a fixed plane;* and I afterwards found that I could *apply* quaternions to such a conception; but nothing very interesting came out of it, that I remember; and generally in the *extension of double algebra*, which I am *now* aiming to make, *as if* we knew *nothing about quaternions*, I propose to select, if possible, *no one direction, of plane or line in space, as eminent* above the rest. In short, I want to recognise space as being entirely symmetrical. The unit-cube, with its three edges from one corner, *marked* as 1, 2, 3, might be tossed about in the air, and would still serve, in all its new positions, to determine, as well as at first, the only data of the sought system. It is only the ORDER *of cyclical* succession of the three rectangular directions which is important, and not those directions *themselves*.



'Conceive, now, that *BACD* is any rectangle in any plane, and that *AE* is a line of any length, erected perpendicular to that plane at *A*; and let us seek, by imitating as far as we can the processes of double algebra, to *interpret the expression* $\frac{EB}{EA} \cdot EC$. Since $EB = EA + AB$, it is natural to write $\frac{EB}{EA} = 1 + \frac{AB}{EA}$; and since $EC = EA + AC$, our expression becomes $EC + AB + \frac{AB}{EA} \cdot AC$; where $EC + AB = EC + CD = ED$: but the *remaining part* $\frac{AB}{EA} \cdot AC$ of the *transformed expression* $\frac{EB}{EA} \cdot EC = ED + \frac{AB}{EA} \cdot AC$, or *the fourth proportional to three mutually rectangular lines in space*, is a

symbol or expression *sui generis*, which Double Algebra wholly fails to interpret, and which, indeed, *cannot be interpreted as denoting any vector*, or line having *any direction whatever in tridimensional space*, if we still resolve to treat space as having *no eminent axis* of direction. Accordingly, I showed in 1844, in the *Proceedings* of the Royal Irish Academy, but better since in the Preface to my *Lectures* (pages (54) to (60), paragraphs [57] to [60]) that the 4th proportional last mentioned ought to be considered as a SCALAR; and that the *conception* of it *as such* is a sufficient basis for a *Theory of Quaternions*. My recent *addition* to the *explanation* is merely the very simple *construction* given above for the VECTOR PART of the 4th proportional to the three lines EA, EB, EC, under the conditions just now stated. I see how to derive from it the construction given in my Fifth Lecture for the vector part of the quaternion, which is, in my system, the value of *the 4th proportional to any three given lines from one origin*, when those three lines are neither in one plane, nor rectangular each to each; but I shall not inflict the process on you, although it has been quite *mentally* performed, unless and until I shall be informed that you, or some pupil of yours, may be disposed to examine this *method of transition* from double to quadruple algebra.'

From the SAME *to the* SAME.

'OBSERVATORY, *December* 22, 1857.

'I asked at Thom's office in Dublin, yesterday, for the last Blue Book, or Parliamentary Report, on the Decimal Coinage; but they had not received it. What *fun* there is in Herschel's answers, as reported in the *Athenæum!* I had hardly thought that he was so witty, although I have long known him to be a very genial person.'

From the SAME *to the* SAME.

'OBSERVATORY, *December* 23, 1857.

'. . . About diverging series, you know a great deal more than I do. In fact you are aware that I early conceived a sort of prejudice against them, in consequence of some of Poisson's remarks. Counter-remarks of yours had staggered me, but had not been carefully weighed. At last (and, I regret to say it, without having yet found the Papers by you and Stokes on *such* series, for Stokes, or Adams for him, sent me about a month ago a duplicate

of his memoir on the numerical calculation of the values of certain definite integrals, having a great affinity to my last Paper) I am become a convert to those Divergents; so far at least as to be satisfied that in an extensive class of cases, and with suitable limitations, they may be safely and advantageously used.'

'*December* 28.—The weather is portentously mild here: new flowers coming out, and birds apparently thinking of building.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*January* 1, 1858.

'. . . I go back to yours of December 14. I have been long under the suspicion that triple algebra is a many-(perhaps infinite-) headed monster—including all kinds of things—quaternions among the number. I follow your explanation haltingly, and am prepared for anything. The *invention* of a symbolic calculus is a large step—very different from playing at inventing one we knew already. I have been looking at Argand (in Gergonne) lately—and I find that if Cauchy took anything from Argand, he took *all*. For Cauchy's proof of the existence of *a* root is Argand's, much complexed, perplexed, retroplexed, and omniplexed. But nevertheless I do not think Cauchy had really read Argand. I think it likely enough that the term *modulus* was introduced by more than one, and that Cauchy heard it casually used. Cauchy was such a master of the symbols that I think if he had been really pillaging Argand, he would have disguised it *more*. Whereas, a common reader, with both before him, sees the sameness clearly enough. Again, Cauchy was a man who sacrificed everything to his principles more than once in politics and bread-winning. I have found this often. A man has much higher principle in his high pursuits than in ordinary life. Witness Newton, Laplace, and a host of others. But I never found such a thing as a man who could sacrifice to principle in the lower things being tricky in the higher. *

'Now to yours of December 23. The French, when they began to find out that they had made a mess of analysis with their

* See a very interesting memoir of Cauchy, presumably from the pen of Prof. De Morgan, in the 2nd Supplementary Volume of the *Penny Cyclopædia*. Cauchy died May 23, 1857.

generalisations, turned round, like angry schoolboys, on the divergent series, and said, it is all your fault, you wicked infinites in a finite disguise, pretending to be finites in an infinite disguise. Of all the jobs I have ever had, the most perplexing has been the arriving at clear notions upon this point. But I have got them at last.

'The truth is that the series in common use may be divided into four sorts:—

'1. Progressing and convergent, $1+\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$

'2. Progressing and divergent, $1+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+\ldots$

'3. Alternating and convergent, $1-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+\ldots$

'4. Alternating and divergent, $1-2+3-4+\ldots$

'1 and 2 are generally separated by the simple infinite, which you may call convergent or divergent. I am content to call $1+1+1+1+\ldots$ convergent in form. Its arithmetical value ∞ agrees with that of the function which gives it, which is always $\frac{1}{0}$, except as hereafter qualified.

'3 and 4 are separated by the neutral form $1-1+1-1+\ldots$ a series tending to it, as $\frac{2}{1}-\frac{3}{2}+\frac{4}{3}-\frac{5}{4}+\ldots$

'Now the blame, suspicion, and distrust, which were thrown upon *divergent series*, belong to progressing series, *per se*, whether converging or diverging. And alternating series, even if diverging, are safer than progressing series, even if convergent. In fact, $0+0+0+0+\ldots$, even when the residual form of a convergent series, is not always 0. But $0-0+0-0+\ldots$, even if the residual form of a divergent series, *is* 0.

'Say anything you like of $\frac{0}{0}$ and I say the same of $0+0+0+\ldots$ I suspect all progressing series, except when I know the producing function. And I trust all alternating series even when I do not know the producing function. And I find results safe.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *January* 4, 1858.

'. . . About Cauchy I am very willing to receive any favourable testimony, or expression of opinion, since I admire so much his mathematical talents. I had heard of his making sacrifices, through his devotion to Henri Cinq. *If* I go on with my definite integrals, I must become better acquainted with several of his writings. *One*

object that I have had in view, in taking up the matter at all, has been to *prepare* myself for attempting, though probably not this year, to do for definite integrals in *quaternions* something *like* what he has done for such integrals between imaginary limits of the usual kind; but I suspect, from some few trials, that the questions will be far more difficult, relating to *curves in space* and to conical motions of a vector. You know that I have published some investigations by quaternions respecting *families of surfaces*, which bear a great analogy to Monge's researches on that subject, and form (as I conceive) the germ of a new Calculus of Partial Differentials, with geometrical applications. And I investigated by quaternions, with reasonable ease, last summer the (known) class of curves in space for which the ratio of the two curvatures is constant. . . .

'If I go on to write about the possible transition, or one mode of such, from Double to Quadruple Algebra, it will be merely as an *amusement*, and probably in letters to John Graves. . . .

'. . . I fully agree with you about the great importance of what you call "alternating series," and think that those lately employed by me have nearly all been such—certainly all of which the law was at all simple . . . As to the *doctrine* of series, I have little or no doubt of your being right. . . . Your remark about Argand's investigation respecting roots of equations, which I never had time to read, reminded me that I had said something hastily, which might seem depreciating as regarded Mourey's analogous investigation. I thought the latter, which I *did* read (in 1854), quite satisfactory, so far as concerned the existence of *one* root; and that he ought to have stopped there, and referred the *rest* to *algebra*. But he went on to make the superfluous effort, in which I thought he failed, to prove *all the* n *roots by geometry*.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*February* 20, 1858.

'. . . I have my hands full of organ-tuning paper and logic.* I have a Paper nearly in proof which winds up with your namesake. The want of mathematics has made the logicians too

* Papers published in the *Cambridge Philosophical Transactions*.

mathematical: that is my text. The world at large will be metaphysicians, and always were, especially the uneducated world. It is essential then that, whether their metaphysical premises be true or false, their metaphysical logic should be sound. A great many persons who think they see that reason is essential to the nature of man, are not logicians enough to avoid *inferring* that it is repugnant to the nature of brute. A great many men have clear ideas of the terms *essential, repugnant, independent.* But how many clearly apprehend the following proposition? "The essential of a repugnant of any notion is independent of that notion." Here is the act of mind of a certain syllogism rendered into those metaphysical terms which are in constant use as indicating modes of relation connected with the *rerum natura*, whereof it is of humanity to know nothing, and to talk as if it knew something. *Quære*, if people will talk about the *ignota*, and *for ignota*, should they talk logically or illogically? Does it matter? I think it does. People actually set fire to each other about "substance" and "person." *Quære*, was it because they knew nothing about the premises, or because they made an illogical use of them?

'Give me the name of an Irishman, or two, who take interest in organ-tuning and beats. I want to promote a knowledge of my plan of having a *whole octave of tuning-forks* tuned by beats, instead of going to work on every instrument by ear on every occasion.'

[In a letter from De Morgan, dated March 1, 1858, he mentions 'a very common theorem' of which he gives a diagram, and of which he says it 'expresses the fact that Pascal's theorem can be obtained, for a circle, *by projection*, without projecting the circle into anything but a circle.' He adds, 'If the point of view of Apollonius had been *projection*—the cone a *means*, not an *end*—he would have had Pascal's theorem.'] .

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *March* 3, 1858.

'. . . I think that when I was hunting years ago for things about Argand and Français in Gergonne's *Annales*, I met some very short proof of Pascal's theorem, in which Pascal's line was,

as in your figure, thrown off to infinity; but the *circumscriber* of the hexagon was a *circle* . . . In the notation of Moebius (Barycentric Calculus) your form of a case of the theorem of Pascal would be thus proved . . . *March* 4.—Do you know much about Moebius and his works? I wish that I knew more, but I do know something of him and them. He seems to be a charming fellow, in his personal character, not that I know any gossiping details about it. He wrote to me, a couple of years ago, and told me that he had been lecturing on my theory of the circular hodograph, to which he might, very plausibly, have put in a sort of claim, or at least a claim to the general *conception* of the *hodograph;* as I admit from a *subsequent* perusal of his *Mechanik des Himmels*, to which very clear and interesting work he had the modesty not to allude. But anything like my "theorem of hodographic isochronism," designed to replace Lambert's theorem, which has not long since been discussed and proved by more than one eminent English mathematician (my own proof remaining still unpublished), does not seem to have occurred to Moebius; and in fact, he makes no claim *at all*, against *me*, though I think that he plausibly *might*. So the world of geometers (in the French sense) is not entirely composed of people "seeking their own."'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*March* 7, 1858.

'The ill-used theory of projection, the original Apollonian method—though Apollonius thought of the cone more as an end than a means—is by far the simplest mode of bringing the conic sections before a beginner, and the clearest.

'And projective properties are by far the easiest and most powerful transformations in existence.

'And Pascal's theorem is the fundamental instrument—I mean, ought to be—of examination of conic sections. For nothing connected with a curve can be more fundamental than the law under which we pass from a determinant number of points to the rest.

'Now to get Pascal's theorem, for the circle, easily, and by help of projections. . . .

'Hence Pascal is fully established. A few propositions of very easy deduction start the beginner with Pascal's theorem, and the whole theory of polar lines. If ever I should write a geometry, I should make wonderfully short work of the whole system. Pascal's theorem gives the $\frac{x^2}{a^2} \pm \frac{y^2}{b^2} = 1$ equation at once.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*March* 23, 1858.

'Your two sheets [of the long letter of Feb. 15, on Definite Integrals] received. The whole will be charming work for the Easter Vacation, as per glance given.

'But how about your son? [Referring to a P. S. of Hamilton's, "My son (Archibald) received a sabre-stroke" in the affray between the police and the College lads.] I hope he is not really hurt. A sword-stroke may be anything from abrasion to decapitation. The attack seems to have been a very unjustifiable one; and, if so, I sincerely hope the quiet part of your population, who have never followed Smith O'Brien or O'Connell, will take it up firmly and continuously. A police officer always gets an *esprit de corps* which is occasionally dangerous.

'Let Lord Eglinton get up a—how do you spell it?—shillaly tournament—and invite all who want broken heads to come and get them in a regular way. The tournament might be kept open night and day, and all the sons of Erin who feel mouldy for want of a beating might get a chance for 2*d.* a piece, which would pay for the viceregal court, over and above the expenses of the institution.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *March* 30, 1858.

'Your construction of the hyperbola by points is very pretty, and I had not met with it before. Your Euclid proof is very clear, and a nice little investigation by quaternions suggested itself immediately to me; which perhaps I may insert in a postscript. Meanwhile I shall mention, what, however, you may easily have

seen for yourself ere now, that the construction can be derived from Pascal's theorem, and can be adapted to the other conic sections . . . [He then gives an extension of De Morgan's rule, and proceeds:] I have not turned to any book to try whether this construction is in print; but suppose that persons who make geometry their profession are well acquainted with it. I imagine that Pascal may have known it himself, although it only occurred to me a minute ago in thinking of your elegant case of it.

'I was very glad to find that you agreed with me in thinking so highly as you do of the importance of Pascal's theorem, as *fundamental* in the doctrine of the conic sections. Your note on that subject is not just now beside me, but I intend to look for it. Some of the geometrical transformations appeared to me to be very ingenious. Have yon ever thought of publishing anything in pure geometry? . . .

'While ranking Pascal above Chasles as an inventive genius, I must still recognise the vast importance of the Anharmonic Theorem of the latter writer, in all that regards the metric properties of conics.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,

'*April* 1, 1858.

'I *got* it from Pascal—from your very construction—and was greatly shocked and disgusted to think that such a fundamental method should have come out of a hexagon with *four* of its points at an infinite distance. The any-two tangents and their pole glanced off as a thing of less use. Except in the case of two tangents at extremity of diameter, which I used, and which ought to be the method of constructing parabola and hyperbola by points. But I never saw either. Parabola of course as easy. Pascal saw all this no doubt. Tradition says his MS. was a treatise on conic sections with 400 corollaries to one proposition. This must have come in the first 400.

'The above gives the easiest ways of deducing the equations which Newton writes

$$PV.\,VG : QV^2 :: CP^2 : CD^2,$$

and the moderns

$$y^2 = \frac{b}{a^2}(a^2 - x^2, \text{ or } x^2 - a^2).$$

'When I was an undergraduate, it happened to me to get very jolly in company with a party who were celebrating the new scholarship of our host. Being, as aforesaid, merry, we proceeded to sing; when it struck one of our party that we could sing as well as the choristers, a notion which came of punch and not of reason. To test the point we all got our surplices, and stood round the table, when a question arose as to what we should chant. Some one proposed $PV.VG : QV^2 :: CP^2 : DC^2$, which met with approbation. We tried to make it fit all manner of tunes; I remember "Zitti Zitti," "the Evening Hymn," and "The Campbells are coming." But we left off with a notion that Newton was not so easily set to music as we thought. We had, however, the satisfaction of knowing that the point had been fairly tried.

'Whewell sent me the other day as a puzzle, to make all the letters of the alphabet into words without omission or redundancy. No letter out. No letter twice. 1. Allowing $j = i$, and $v = u$. 2. Rejecting v and j. The first I did and made the words make a kind of sense, as follows:—"I, quartz pyx, who flings much bed." Such a pyx was never thought of before, I dare say. The second I do not believe practicable. Whewell did it in what I call an evasive way, for I contend that the initials of Latin words are not English words. "Phiz, styx, wrong, buck, flam, Q. E. D."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 6, 1858.

'. . . Do you imagine that Whewell would care for some specimens of my cypher, about which I wrote to you a few years ago, and with which you declared that *you* would have nothing to do? I am very curious to know whether it could be decyphered, though it may be very presumptuous in me to expresss such a doubt. At all events it *can be read*, and it can be, for it has been, *communicated*, though only to *one* person hitherto.

'I never pretended to be able to do more than the *very easiest* tricks of decyphering, myself; such as making out the meaning

of a moderately long English paragraph, when the words were all divided, and each letter of the alphabet was replaced by some one fixed symbol.'

From the SAME *to the* SAME.

'OBSERVATORY, *Easter Eve* [*April* 4], 1858.

'I had a visit to-day from my old friend, the Rev. Robert Perceval Graves, brother of John and Charles. While he was here I wrote out for the latter the two following theorems: will you tell me whether you think that they are known? —

$$1.\quad f(x+h) = fx + hDf(x-h) + \frac{3^1h^2}{1.2}D^2f(x-2h) + \frac{4^2h^3}{1.2.3}D^3f(x-3h) + \frac{5^3h^4}{1.2.3.4}D^4f(x-4h) + \ldots + \frac{(n+1)^{n-1}h^n}{\Gamma(n+1)}D^nf(x-nh) + \ldots$$

$$2.\quad f(x-h) = fx - hDf(x-h) - \frac{1^1h^2}{1.2}D^2f(x-2h) - \frac{2^2h^3}{1.2.3}D^3f(x-3h) - \frac{3^3h^4}{1.2.3.4}D^4f(x-4h) - \ldots - \frac{(n-1)^{n-1}h^n}{\Gamma(n+1)}D^nf(x-nh) - \ldots$$

fx is here as arbitrary a function as in Taylor's series, and $D = \frac{d}{dx}$. I think that I should remember the *look* of the equations, if I had met them in any book.'

'*April* 6.—Thanks for your note of the 1st, which reached me yesterday. I have not yet considered your last construction, but should have been surprised if you had not anticipated my remark about the former case of Pascal. About Geometry I may write after no long time—or I may not. Meanwhile let me say that I have communicated in a note of this morning, to Charles Graves, the following theorem, which includes the two lately given, a being here an *arbitrary* quantity, which may if we choose be

imaginary:

$$3. \quad f(x+h) = fx + hDf(x-ah) + \frac{(1+2a)^1}{1.2} h^2 D^2 f(x-2ah) + \frac{(1+3a)^2}{1.2.3} h^3 D^3 f(x-3ah) + \ldots + \frac{(1+ma)^{m-1}}{\Gamma(m+1)} h^m D^m f(x-mah) + \&c.$$

C. G. did not *remember* either of the two cases lately sent, in one of which a was = 1, and in the other = − 1; h having also been replaced, in the second case, by $-h$.

'I shall be curious to know whether *you* ever met the theorem, just now written, or even either of those cases of it.

'The *oddity* of the *look* of a series for $f(x+h)$, with a *foreign* and *arbitrary constant* (or variable) quantity entering into it, makes me think that I could not have failed to *recognise* it, even if I had forgotten the result, if I had ever seen it in print. You will find the investigation in the eleventh sheet of my long letter, not yet ready to be posted.'

'*April* 10.—Robert Graves dined and slept here on the 8th, and we drank the health of Lady Hamilton on the 9th—that day, yesterday, having been my *silver* wedding-day; for I was married on the 9th of April, 1833. He (R. G.) had the audacity to wish me a *golden* anniversary of the same event; but I said *out*, before my wife and children, God forbid! It would in fact be frightful to anticipate the bare possibility of living so long.

'As to my recent equation 3, after finding it for myself, I felt a dim sensation, or a shadow of a recollection passed before me that *something like it* had been published by Laplace, in his Calculus of Probabilities, near the beginning; or at all events a development in which a *third arbitrary quantity* was introduced, in an expression for a function of the sum of only two such quantities. Accordingly, I observe that in page 14 of the *Edition de luxe* of Laplace's complete works, published at Paris in 1847 (at least the seventh volume was so), and therefore, during the reign of Louis Philippe and anterior to the present regime, the following formula is given:—

$$y_{x+1} = y_x + i\Delta y_{x-r} + \frac{i(i+2r-1)}{1.2} \Delta^2 y_{x-2r} + \frac{i(i+3r-1)(i+3r-2)}{1.2.3} \Delta^3 y_{x-3r} + \&c.;$$

which has a very decided *analogy* to my result, but, as you see, introduces *finite differences* instead of *differential coefficients.*

'I have noticed—but forget the page—that Laplace speaks also of "series limités," in reference to such divergent series as give alternately values erring in defect and in excess."

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 11, 1858.

'I have a long retribution for you when I find time. I thought I remembered something like your theorem somewhere in Murphy; and, on looking up, I find in his *Theory of Equations*, p. 82,

$$f\overline{x+k} = fx + \frac{2k}{2}.f'\overline{x-k} + \frac{\overline{3k^2}}{2.3}f''(x-2k) + \frac{\overline{4k^3}}{2.3.4}f'''(x-3k) + \ldots$$

with reference to a memoir on analytic operations in the Royal Society's *Transactions.*

'As to twenty-five years more life—why not? If it would be frightful to anticipate it, don't anticipate it, but live on quietly. The clock mutinied when it anticipated the number of times it would have to tick in the coming twelvemonth, but was reduced to order by being reminded that for each tick it would have a second to tick in.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 14, 1858.

'I take the first sheet of note-paper that comes to hand to tell you that I am much obliged by your having pointed out to me Murphy's anticipation of at least *one* of my theorems, or say *two* of them, with exactly the same filiation as in my long letter. . . . Consequently I see that I have *no claim* to having been the first to perceive this transformation of $f(x+h)$, and think it lucky that I did not *print* it. *As yet,* I do not know whether Murphy (whom I knew a little, but whose book on Equations I have never read) was aware of the still *more general* series,

$$f(x+h) = \sum_{m=0}^{m=\infty} \frac{(ma+1)^{m-1}h^m}{\Gamma(m+1)} D^m f(x-mah),$$

which was number (370) in a sheet lately forwarded (the eleventh),

and in which there occurs an arbitrary contrast, a. But I admit that Murphy may very easily have been led to perceive, and even that he is *likely* to have published it. At all events, I must take care not to print any of *these* results of mine, without previously endeavouring to ascertain how much he had done on the same subject. At this moment it is my impression that he had *not* seen the *three* real and unequal roots of the transcendental equation $a^{a^y} = y$, (402), for the case $a > 0$, $< \varepsilon^{-\varepsilon}$, which are discussed in sheet xii. of my letter, lately posted. And other transformations may still be mine, although I should be cautious of asserting them to be so.

'Sheet xiii. is almost copied, and will probably be posted to-morrow. It will conclude the discussion of the series above cited from Murphy (but which I had till to-day supposed to be my own), in connexion with the *two methods of transformation*, by continued fractions and by continued exponentials. Sheet xiv. will break ground in a partly new direction, by bringing in a *third transforming function*. And on the whole I trust that we are at last in sight of land.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*May* 16, 1858.

'I am *au retard* with your MSS., and other things besides, by reason of other business. But I take up now and then, and get along somehow; and I shall write you something about it. When the Beats* were off my hands came on a Paper on Logic, the which, being very saucy to the logicians, must have great looking over, that I may not lay myself open. In fact I have thrown it in their teeth that a man must be a mathematician before he is competent to understand the distinction of *form* and *matter*, for which I think I give reasons out of the nature of the case and the palpable slips of very acute logical writers. . . . I am now in possession of a tolerably complete idea of my own as to what logic should really be, in its two sides, *mathematical* and *metaphysical*. And as I am more than ever *aux prises* with your namesake, I wish he were alive to have a touch at me. But as that cannot be, the next thing is to wish him an editor or an old pupil who will take up the cudgels for him with more judgment and temper than he could do for himself.'

Supra, p. 542.

From Sir W. R. Hamilton *to* A. De Morgan.

'Dublin, *May* 26, 1858.

'Whatever else I may write about to you, this "Letter" (of Feb. 15th!) is at last finished! and I have the satisfaction now of posting the last sheet of it. . . .'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*June* 3, 1858.

'I have actually READ through (not *worked* through) your letter of 72 folio pages, begun on a sheet which you said you did not expect to finish. Did ever mortal do such a thing in hot weather? Why, it is a collection of memoirs. Most truly did you begin by saying that an inventive mathematician need never fear being in want of a stock of difficulties. . . . You have signalised yourself by a real inroad into the territory of divergent series, in which I have latterly believed as much as in convergent series. Not that I have believed them more, but that I have believed convergent series less.

'Your numerical treatment is wonderfully successful, though mind, it is but a beginning. I should almost have grudged your taking the trouble for me, if I had not been satisfied that it was the best thing you could do. You will find that you pick up in all directions when you begin to write your memoir.

'But, Heaven and Earth! how is a man to carry it all in his head? Thank goodness that I have not to *get it up* for an examination. There is so much novelty about the forms that the old things do not help.

'You have added evidence to my *dictum* that it is not *convergency* which is safety in series, but *alternation*. And depend upon it that some day it will turn out that the safety of alternation lies in $(-1)^{\infty} = 0$, ∞ being an infinite integer—not restricted—not even, nor odd.

'There is a nibbling, too, at the next step in the progression of operations

$$a+a+a+\ldots \quad a\,.\,a\,.\,a\,.\ldots \quad a^{a^{a^{}}}.$$

'It may be that the inversions of this function will one day be as well understood as those of $a.a.a...$ What grand people they will be who know all about this.

'I must await the long vacation to have a pull at your series again. . . .

'I have been screwing away at logic, and shall soon have the Paper printed. I have now made up my mind about that subject, and am above all argument, a perfect fifth monarchy man. The thesis is that the logicians—unwittingly—are too much of mathematicians; that they have been wandering hopelessly in the mathematical field of logic; and that they want a mathematician to show them the way out.

'Should I draw up a tract on the subject for more general circulation, I shall preface it with a motto from Hobbes, as overleaf. It will be a cool thing to adopt the language of his unfortunate attack on the geometers; but I intend to *be* cool. "In magno quidem periculo versari video existimationem meam, qui a 'logicis' fere omnibus dissentio. Eorum enim qui de iisdem rebus mecum aliquid ediderunt, aut solus insanio ego, aut solus non insanio; tertium enim non est, nisi (quod dicet forte aliquis) insaniamus omnes."

'Your namesake has a train of thought about quantity, in which he begins by asserting that two quantities are "one and the same" quantity. Three sentences further on, he says it is evident that these quantities are always each the *inverse* of the other; the greater either of them is, the less is the other. Equal quantities varying each inversely as the other make a difficulty greater than divergent series. The key to all this is that he never abstracted quantity, in his own mind, from its subject of inhesion, and thought as grossly of quantity as a shopkeeper who says he has put the *same quantities* up together, when he has packed up again in the same parcels those lots which came in in the same parcels. Strange, but true.

'Really this weather is boiling and roasting all in one.'

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*July* 27, 1858.

'I have just got three sheets [of the long letter of July 15], but as yet have been only cursory. The Paper on Logic has been my hindrance. Look at the appendix, and see what grand notions Sir W. H. has of *quantity*.

'My theory is this. What's in a name? Ans.—To a given name nature has but a certain amount of anything to dispose of. If one Sir William Hamilton takes up an enormous share of the mathematical power disposable among the lot, the others must suffer for it. Now the Edinburgh man had a very strong mind which required a very large quantity of mathematical power to balance the other capabilities. But another of his name had *shared him out*, as sailors say of the messmate who drinks nearly all the pot of beer before the others get a chance.

'I dare say Cayley will send you—or has sent you—his Report on dynamics. You will enjoy it.

'I am now deep in the logic of the word "of"—a totally unexplored part of the subject.

August 17.—'I have got your three sheets, which I have run through, and have them before me, to have another go at them, when a certain job, of which you will see the results, is finished. You are getting on famously. I have not had gossiping time for months and months, much owing to my logic, which I hope you received. I have since been occupied with the syllogism in which *any relation* is the copula, and with success. It throws on the common syllogism the light which $y = \phi x$ throws on $y = x^2$ or any other instance. Now as all my people are at the sea, I have a little time to trifle. And I proceed to the subject of anagrams. One day, weeks ago, my wife sent into my room a bottle to be labelled POISON, in the doing of which I saw "O! no sip!" which I added. This warned me to look at any word I had to discuss, to see whether transformation of letters would enforce any argument. A few days ago, I was writing for *Notes and Queries* about Newton's apple, which had been produced by a contributor with the usual implication that no one had ever thought of gravitation until Newton saw the fruit fall. So I brought forward Kepler

and his sun-and-planet gravitation, with force inversely as the distance; Bouillaud with his assertion that if gravitation there were, it must be as the inverse *square*, and that the earth ought to attract the moon; as well as Huyghens, with his law of connexion of velocity and force fully stated for the circle; all known to Newton. Then I began to think of the anagrammatic verification, and I found solid arguments in the transposition of the letters as in the opposite page, which will convince anyone who has a theory of correspondences, be it Swedenborg's or any other. You are a fortunate man; you only fall short by a letter of *hot animal*. I transmogrify into *mad negro*. Newton, as to primary ideas *not new*, but as to all else *went on;* in perceptions, *no newt;* as to reputations, *won ten*, that is, *ten now*, not being fully appreciated in his own time. This is as much as we could expect of so small a number of letters.

'If you have access to Newton's letter to Halley, June 20, 1686, as in Brewster's Newton, i. 440, Rigaud's *Historical Essay*, Appendix, p. 28, tell me what you think Newton means. Does he imply that he *had* the propositions about the circle when he saw Huyghens's book, or that he was ready, on seeing them, to apply them?

[After giving an instance of a syllogism implying *relation* in its copula, he proceeds:]—

'This is one of the questions I have been studying, and though very easy when it is done, it is really difficult. The pitfalls are numerous; and if it had not been for the familiarity with relations of consanguinity and affinity which every man has, I should have been resting in blunders of more than one kind.

'Not that all the compositions and decompositions of these relations are familiar. When I was a boy I have seen plenty of people puzzled by the following:—An elderly nun was often visited by a young gentleman, and the worthy superior thought it necessary to ask who it was. "A near relation," said the nun. "But what relation?" said the superior. "Oh, madam," said the nun, "very near, indeed; for his mother was my mother's only child." The superior saw that this was very close and did not trouble herself to disentangle it. And a good many of the people to whom it was proposed used to study and bother to find out what the name of the connexion was.

'If we take the following case of the preceding syllogism:—

X is the *uncle* of Y,
Z is not the *cousin* of Y;

we see by practice that

X is not the father of Z.

But what is required is a scientific mode of detecting the conclusion.

'I hope you and family are all well. The Dublin air is, I suppose, unexceptionable. At least I never heard anything against it. I believe and maintain that London air is as good as any air; and as to the trifle of smoke, it is nothing—a natural cigar, nothing more.

'By the way, here is the analogy of $y^2 = x^2$ $x = \pm\sqrt{y^2}$.

Y is the brother of X,
X is the (brother or sister) of Y.

'Here is another quadratic equation. A broadside quoted by Macaulay, describing the death-bed of Charles II., says that the D(uke) of Y(ork) was reminded of the duty of bringing a priest to his brother by P. M. A. C. F. Now the Duchess of Portsmouth instigated the Duke of York, and Chiffinch, the pandar, smuggled the priest into the palace. One solution is that the initials are syllabic initials of Ports Mouth And Chiff Finch. But it has lately been remembered that the Duke of York's confessor, a very likely person to remind the Duke, or to be said to have reminded the Duke (which will do as well for a broadside concocted from rumours) was Père Mansuete A Cordelier Friar. The question is, which root of the difficulty must be taken?'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *September* 13, 1858.

'I received your note of August 21, but started almost immediately afterwards for St. Mary's Abbey, Trim, where my dear daughter Helen was on a visit to some cousins of mine, and was then dangerously ill. *September* 14.—I left her, however, after a visit of about three weeks to those scenes and friends of my boy-

hood; my surviving cousins indeed had not been born when I used to be most at Trim, but Dean Butler and Mrs. Butler (who was Miss Harriett Edgeworth) are two of the very oldest of my living friends, and when I came away, my daughter was much better, and all danger was considered to be over. . . . After a little while it may amuse me and not bore you if I resume the subject of my last long "letter," and wind up with a seventh sheet.

'*October* 13. . . . I was writing mathematics all the morning, but your seventh sheet hangs in the clouds of expectation. I was completely diverted from the subject by my daughter's illness in the first place, and lately by a correspondence into which I have been drawn with a very intelligent man, a Professor of Mathematics (but not in this University) and an author, who has been most conscientiously studying my book and has consulted me on some points of practice, and some of theory, especially as regards the *finite differentials* (not differences) of *functions of quaternions*. I have just been reading (thanks to your "flapper," Halley) the 2nd Lemma of the 2nd Book of the *Principia*, and conceive that my differentials are just what Newton would have called *fluxions*, though modified by the nature of the applications.

'Am I correct in my impression that *my definition of the differential of a function* (*Lectures*, Art. 568, p. 569) struck you, *at the time*, as a *new* one? And if so, have you since met with it elsewhere?'

To this inquiry De Morgan replies, December 6:—'I have looked at p. 569, but am not able to remember what I thought at the time. I think it very likely I remarked that the form was a new one.'

[In a letter dated January 3, 1859, Hamilton, reverting to the subject of Definite Integrals, communicates to De Morgan some additional conclusions which he says may be considered as 'in anticipation of, perhaps in substitution for,' the seventh sheet lately promised by him as an extension of his letter of July 15, 1858. His subsequent letters of 1859 are numerous, but being taken up with an application to De Morgan, as Secretary of the Royal Astronomical Society, for a grant from the Society of its Monthly Notices to the Hon. Mrs. Ward (*supra*, p. 104), with the

proposal of problems in his Icosian Game and Calculus, and with requests for criticisms on the early chapters of his *Elements* (then called by him *Manual*) *of Quaternions*, do not afford much matter for publication. I extract a few passages of general interest.]

From the SAME *to the* SAME.

'OBSERVATORY, *March* 11, 1859.

'Your last note is not beside me, but I was reminded of it to-day, by receiving, through the post, a Monthly Notice of the R. Ast. Society. Do you really write the whole of those formidable but interesting reports?'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'*March* 27, 1859.

'I do not write much in the Annual Reports. I *edite*, and I part out my subjects to contributors. Main did Donati. As to my doing such a thing, about observations and so-forth, I can only say what one of my little boys once said when his mother told him he must love his enemies—Catch me at it!'

[In the early part of this summer De Morgan changed his residence from Camden-street, N. W., to 41, Chalcot Villas, Adelaide-road, N. W. He thus dates the first letter written by him after his removal]:—

'41 $\left\{\begin{array}{l} x^2+x+41 \text{ gives nothing} \\ \text{but primes from } x=0 \\ \text{to } x=39 \text{ inclusive.} \end{array}\right.$

'CHALCOT VILLAS.—All the world knows Chalk Farm. There is no Chalk in London clay, and Primrose Hill, close by, is a clay pudding which would be of impossible form in a chalk formation. *Chalk* Farm is *Chalcot* Farm, corrupted. (See Camden.)

'ADELAIDE ROAD.—*Addlehead* may suggest.

'N. W.—I never knew points of the compass. I always remember this by thinking that I live No Where.

'Now if you are prompted I will go on.

'I think no one would imagine either from your letter—or

Mr. Smith's [of circle-squaring notoriety] comment—that you were in any other π than 3·14159265 . . . . Have you any objection to send your short proof to the *Athenæum*? . . .

'*October* 6 . . . I am sincerely rejoiced to hear of your daughter's total restoration. She must now consult the Friendly Societies' table, and she will find that she has had more than her share of sickness, and as nature always preserves her sum-totals, she will see that she cannot have any more without robbing somebody else, which I am sure she will be too right-minded to do.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *October* 9, 1859.

'. . . I am aware that I use *italics* and CAPITALS too freely: perhaps it may be left to the printers to correct that fault, or possibly a reader may bear with it, as being in some degree *characteristic*, and as indicating a sort of *vivacity*, if it be too grand a word to say *earnestness*, on the part of the writer.

'I will tell my daughter what, as an *Actuary*, you say; and count on its being *useful*. As a boy I was fond of going into dangerous places; nor did marriage at once tame me; but when I came to *insure my life*, I thought it was *not honest* to risk my neck any longer.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'41, CHALCOT VILLAS, ADELAIDE ROAD, N.W.,

'*October* 11, 1859.

'. . . Trusting you are now old enough to be in no danger of a relapse, I will tell you that you are not bound to the office to restrain your natural temperament. Their tables are calculated on an average of *all sorts*, and your reckless people are all down among the earlier deaths, and the premium is duly augmented by them. If indeed, having been a cautious man up to the assurance, you were then on the faith of the assurance to become reckless, you would, *pro tanto*, be playing tricks with the table and acting unfairly.

'*October* 23.—All right. I don't think you *are* exceeding. You *may* at any moment.

'The principle of elimination? No.

'Elimination is an accident—not the main object of thought.

'I call it the principle of *transition.* [To this is affixed a marginal note by Hamilton—"Adopted, W. R. H."]

$$a = b \qquad b = c$$
$$a = c.$$

You pass from *a* to *c* through *b.*

'When you cross a river upon a bridge your main thought is *to get over.* You do not name your action or purpose from the recollection that, in afterwards thinking of the connection of the two sides, you may forget the bridge.

'The sign = is [may be?] of *transitive* meaning. Fallacies may arise from *transitiveness* not being present in thought. *Ex. gr.*, Tim and Paddy had a cask of whiskey to carry home. Tim carried it a good way, and then, being tired, proposed to Paddy to carry him on his back. Paddy did not see the fairness of this. "Sure," says Tim, "if I carry the whiskey, you ought to carry me—would'nt that be fair and equal?" Tim forgot that the verb *carry* has transitive meaning.

Paddy carries Tim,
Tim carries the whiskey;
∴ Paddy carries the whiskey.

'This all arises from logicians not considering any *copula* except *is.*

'Was Tim *eliminated*? If Paddy had tried he would have found.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *December* 1, 1859.

'You have done *exactly* what I wished and expected you to do, that is, you have given me your *impression* (which you were too polite to clothe in just the words that I am about to write) about my sheets, that they are *tedious;* or, at least, too long.

'I had told myself of the same thing; but just as I feel the sending a sheet to you, now and then, to be a semi-publication, so I accept the gentlest remark of yours as a semi-criticism.

'Your own works are so much admired by me, and you were so many years earlier in the field as a writer of *books*, that I receive any observation, or even *hint* of yours, with a really genuine deference.

'I trust that when my MANUAL comes out, of which I have great hopes that a *part* will, within a few months, you will find that I have endeavoured to *abridge*.'

[The most important letter in the correspondence of 1860 was one dated October 29, which communicated to De Morgan an early statement extending to twelve folio pages of results arrived at by Hamilton on the subject of *Geometrical Nets in Space*. A full treatment of this subject may be found in the *Proceedings* of the Royal Irish Academy, to which body the Paper was presented on the 24th of June, 1861. I extract a few passages from other letters.]

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'41, CHALCOT VILLAS, ADELAIDE ROAD, N.W.,
'*May* 26, 1860.

'If you are dead and buried, why do you not say so at once, like a man, instead of insinuating it in this roundabout way by solemn silence? What has become of you and of the Manual of Quaternions? I write because I want to know something about you. For myself, I am in the most jog-trot state imaginable, doing and suffering nothing particular. . . . If you do not write I shall circulate a report that you have shipped yourself to fight for the Pope. That worthy has recruiting serjeants, I am told, in Ireland, and I may as well dispose of you that way as any other. You know, perhaps, that neither Boswell nor anybody else could ever find out where Sam Johnson was in 1745 and '46. He was a Jacobite at heart; and though nobody ever thought it seriously probable that he had been *out*, yet it was a strange puzzle, for the thing was not impossible. Now you are not a Papist, and therefore the *à priori* grounds of probability are less in your case.

'I had intended to have refreshed my quaternions before this, by help of the manual which you were engaged upon. But *il n'y a pas de quoi*, as the cherub said to the gentleman who begged him to be seated, when he came on a message from above.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Wooden Bridge Hotel (Avoca), Co. Wicklow,
'*July* 9, 1860.

'I am touring for a few days, with my daughter, and with a couple of cousins of ours from England, who happen to be with us at present. The scenery in this county of Wicklow is undoubtedly charming. . . .'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N. W.,
'*November* 6, 1860.

'I got your letter [on Geometrical Nets in Space], and looked a little at the ABC of it, but one must keep these things up to be ready at them, and I have been at other things a long time. However, your second sheet will be helped by your first. How is the quaternion summary going on?

'I am reviewing your namesake in the *Athenæum*. His lectures —very well worth reading—on Logics and Metaphysics were sent to an old pupil of his for review from the editor, but said old pupil being busy, they could not be got out of his hands. So, the books at last being recovered, they were sent to me, and I have accordingly held him up in some things and showed him up in others. His mathematics are perfectly extraordinary. He cannot correct gross blunders about Euclid I. 1., which he quotes from another for his own hearers with approbation. To read his article against mathematics in the *Edinburgh Review*, you would suppose that though he might not have known much, he had a good οἱ πολλοι knowledge. But he has certainly never read I. 1 to understand it. He quotes with approbation the two assertions:—1. That it is *proved* that the three lines drawn *constitute* a triangle. 2. That it is *proved* that the *two circles meet*. Charles Dickens has shown more knowledge of this proposition, when he said that Mrs. Peerybingle's pattens made the diagram all over the yard.'

[In this month of November Hamilton confided to De Morgan the fact of his vexation at seeing an advertisement announcing the forthcoming publication, by Professor Tait, of an 'Introduction to

Quaternions.' In the middle of December Hamilton reports to his friend that in reference to this matter he is '*now quite satisfied.*' He had not objected to Professor Tait publishing '*Examples* on Quaternions,' but an *Introduction* was regarded by him as an interference with his rights in connexion with his own intended publication. He refrained, however, from remonstrance, and was therefore the more gratified at Tait's 'offering to be guided in every respect by my wishes.' I omit these letters, the subject of which has been sufficiently adverted to by me, *supra*, pp. 133, 134, but I may give De Morgan's concluding letter. In part of it he refers to an allusion by Hamilton to a controversy then being carried on between De Morgan and Dr. Mansel.]

From the SAME *to the* SAME.

'41, CHALCOT VILLAS, ADELAIDE ROAD, N.W.,
'*December* 18, 1860.

'I am very glad the affair is so well arranged. I felt quite certain that nothing that could be done, on any supposition, would do you any damage, except as you chose to think so. And it came round to me, circuitously, through people who knew nothing at all about either of you, that Tait is a very strong admirer of yours; so I feel pretty sure that he would do anything you wished in the matter. I am quite satisfied you cannot give him, or let him take, any licence which can damage or *de-priorise* anything you choose to write on your own subject. No man knows except by long study of history how safe a man is on his own ground. Mathematics are not enigmas; at least nobody can do anything, by ploughing with another's heifer, which will make a furrow at all like that which a man makes with his own plough. There is allegory for it; so it must be true.

'Mansel and I have done with each other for the present. We are very good opponents, exchanging private letters all the time, I exhorting him to *pitch in*, and telling him that, since Hamilton's (simplex) death, I was, as the Irishman said when he set out for Donnybrook, "dry-moulded for want of a beating."

'The practice of *logicians* of establishing exact science by

quotations has gone a fearful length when the most *nepial** blunders are excused on plea of quotation.'

[The correspondence between Hamilton and De Morgan in 1861 was much occupied with the controversy carried on in the *Athenæum* with Mr. James C. Smith, the circle-squarer. In this controversy Hamilton's effective part was a communication to the *Athenæum*, recorded *supra*, p. 126. De Morgan had a peculiar pleasure in belabouring this pretender to a mathematical achievement. Out of many passages of his letters answering to this description I select a few which will amuse the reader, and to them I add occasional sallies of a similar character against another favourite foe of his, Sir William Hamilton of Edinburgh. I think it will have been observed of De Morgan that, while intellectually rejoicing in the fight of controversy, there was no malice in his combativeness. I have on this account felt the freer to print these specimens of his trenchant strokes. The letters of the year, however, are not confined to these topics.]

From the SAME *to the* SAME.

'41, CHALCOT VILLAS, ADELAIDE ROAD, N.W.,
'*April* 29, 1861.

'... I hope you and yours have been well all this long time ... Last June I tried pleurisy, and did not like it at all. But it did not grow very serious. You got I suppose my "Relations" (Logic). I am at a fifth Paper. I suspect you have robbed all the Hamiltons of their share *quantitatively*—all the Williams at least. Your Edinburgh synonyme says—*totidem verbis*—that "some at least" includes in its meaning "possibly none," and that therefore Aristotle and all his followers have included in one contrary propositions. Meaning that the "some X is Y" of Aristotle—where "some" is "some at least"—must include "No X is Y." Pretty well for a man who professes that he has "placed the key-stone on the Aristotelian arch." What notion

* I learnt this word [derived from νήπιος] from Halliwell, who used it of an antiquary twice his own age, when he, the antiquary, found in a MS., "As J. C. saith, the poor ye have always with you," and filled up J. C. thus J[ulius] C[æsar].

can he have of an arch, which has stood *without* a key-stone for 2000 years?'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *May* 1, 1861.

'. . . What *is* pleurisy? . . . the *word* is familiar to me, and I regret that you have made even a passing acquaintance with the thing. For my own part I have been really rather *wishing* for a fit of the gout this spring; but it has not come. When I have one—which has happened some few times already—I think that I have borne it pretty well, and philosophically: although the physical pain was very great. But it did not disturb my mind, and scarcely interfered with my writing. And I was so kindly nursed, and looked to, that I have positively quite *pleasant* recollections of the fits.' [*Supra*, pp. 154, 52.]

'*May* 11 . . . On the same Monday I received your note of the 4th, beginning with the mysterious name TEOTA [initials of "The Editor Of The Athenæum"]; which when I explained to Lady Hamilton, she was highly amused, and said that "she would give anything to see De Morgan."'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,
'*May* 31, 1861.

'. . . I have sent your proof to the *Athenæum*—you see Mr. Smith wants a little more killing . . . As to convincing Mr. Smith, you will convert Father Flaherty's pig to Protestantism first. When the animal solemnly abjures the errors of Popery on his hind legs, with his fore paw on the Gospels, then try Mr. Smith. But easiest first, always.

'From fate more than peace enough;
From nature not sense enough;
So circle must go without
Circumference enough.

But I did not stick this into your proof. I am afraid Lady Hamilton will wish to see me more than ever. But how she would be disappointed! I have not a word to say for myself, whatever may ooze out of my fingers' ends. With my respects,

this. There is another Lady Hamilton whom I must have bored terribly; since she had to be amanuensis through many a weary page of writing, all along of me. Look out in the *Athenæum* for more Hamilton controversy. What say you to a man who says that "some at least" is "possibly none"?

'*June* 16.—Will you believe that Mr. James Smith is going to leave Liverpool—where he is great in dock-business—and to come to London; solely that he may bring the astronomers to a sense of the errors they commit in the places, orbits, &c., of the heavenly bodies by reason of their wrong value of π? You need not believe it unless you like. All I can say is that my friend James Brown (whom probably you know) avers that he received a missionary visit from a gentleman named "James Smith," who called on him to announce his intended change of residence and its reason. There cannot be two James Smiths equally bitten with $3\frac{1}{8}$. I don't know how Brown dealt with him. The New Zealanders eat their missionaries. I suppose Brown "chawed him up," in the Yankee sense, somehow or other.

'This reminds me of a rhyme to Timbuctoo—not an easy word—which I heard some days ago:—

> "I wish I were a cassawary
> On the plains of *Timbuctoo*;
> I'd catch and eat a missionary,
> Legs and arms, and *hymn-book too!*"

For myself I could more easily swallow James Smith than his π. I hope he will go first to Airy as in duty bound.

'By the way—this suggests a pun—all my own. Mr. James Smith must now consider Airy as a most *forbidding* personage. "Sir, it would be a waste of time to listen to anything you could have to say on such a subject." And what makes Airy so forbidding? Why, because he has the "mens *say-no* in corpore *say-no*."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *June* 30, 1861.

. . . . . .

'P.S.—It is, I suppose, about a fortnight ago, but it may be three weeks, that I received a note from you, in which you mentioned that you had perhaps caused some trouble to Lady Hamilton of Edin-

burgh by obliging her to act as an amanuensis, &c. I read it out for my wife and daughter; and remarked that my landlady in Edinburgh, about ten years ago, might have thought me a sort of Don Juan if she had observed that I posted, on one evening, notes to *two* Lady Hamiltons. My daughter Helen said, "I suppose, Papa, that you directed them with your *two hands*, on the principle of not letting the left hand know what the right hand doeth." "If you go on this way," I replied, "I will tell De Morgan." '

[In De Morgan's reply he says:—'I fully agree with Miss Helen, that the two hands ought to be employed, and the rather because one of the two marriages must be a left-handed one.']

'*August* 7, 1861. . . . As another joke, however poor it might be, I enclosed to you yesterday the first proof of the demi-sheet 2 K, of my *Elements*, with a foot-note (p. 252) about "purely geometrical sighs*"! My daughter thinks that *you* might be able to assign the form of a "purely algebraical sigh." '

[On a separate page of note-paper, and subscribed with his monogram, but dated *September* 27, '61, I find in De Morgan's handwriting the following "puzzle." It is plain from Hamilton's letter of August 10 that his daughter had previously to that date received from De Morgan an answer to her challenge to supply the 'form of an algebraical sigh:' but no such answer appears in the correspondence. I cannot but suspect that this "puzzle," as a corrected version, was substituted at the date subscribed for the original reply.† Whether it was De Morgan's invention, prompted by Helen Hamilton's challenge, I am not able to say. I have heard a different authorship attributed to it. Certainly it seems to fit into the present context most appropriately]:—

"U O a O but I O thee
O O no O but O O mē
And O let my O thee no O be
But give O O I O thee."

A puzzle. A young gentleman, who stood much in need of algebra,

* "Sighs" a misprint for "signs."

† Yet De Morgan's allusion to his answer contained in his letter of March 11, *infra*, seems to intimate that it was more algebraical than this "puzzle."

was asked by a young lady from whom he desired it how she might best arrange her initials on a seal which she wished to have cut. The answer was as above; and it strongly illustrates a problem once proposed to me in transcendental algebra, of which I sent my solution to the British Association, where it was refused on the ground that squaring matters with a ring was too nearly connected with the quadrature of the circle.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *August* 10, 1861.

'My daughter was in a very pleasant state of anger yesterday, at your having so *per*tinently (not *im*-) answered her question about the *form* of the *purely algebraical sigh!* We have heard the words "*in due form*"; but it has not quite come to *that* with her, *yet*. The little puss has stolen away from me, to spend to-morrow, which will be her twenty-first birthday, with an aunt of hers, about ten miles off, who is my only surviving sister. So, *perhaps*, you may have an opportunity of drinking her health—in water, if you choose.

'P. S.—A few days ago I took down an old large prayer book, and pointedly called her (my daughter's) attention to that Canon of the Irish Church which forbids minors (= people of each sex, *under twenty-one*) to even contract themselves in marriage, without the consent of parents or guardians. She seemed to think that she could bide her time.'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,
'*August* 12, 1861.

'. . . I dare say the young lady's wrath will cool down. It is the technical, official, kind of anger which is always put on against any insinuation that the passive participle of the verb "to captivate" may be their lot as well as the active. *L'un n'empêche pas l'autre, mais au contraire.* I congratulate her on being out of reach of the canon. You waited nearly to the day before you pointed out this canon, as you tell me. It was wise; for there is in the best a principle of restiveness, which delights at setting control at defiance; and not in young ladies only. I have heard of a young

lady who outgrew the canon without knowing it, and who afterwards, at 21 +, hearing of some other who had been prevented from disposing of herself at 21 −, said "I am sorry I didn't know I mightn't: I'm sure I would."'

[In the mathematical letters of 1861 Hamilton submits to De Morgan investigations afterwards introduced by him in a matured form into his *Elements:* De Morgan similarly submits to Hamilton mathematical work in the theory of probabilities applied to errors of observation and also original views in the theory of Perspective. These portions have been thought unsuitable for presentation here; but I give, as likely to be interesting, a statement by Hamilton of his own opinion of the comparative merits as books of his *Lectures* and his *Elements.* The printing of the latter work was now, however, only half-way through.]

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 9, 1861.

'. . . As a *book*, I am far better satisfied with the new volume than with the *Lectures*, which, however, is not saying much, for I am very much *dis*satisfied with *them*, in point of composition and arrangement—the *συντάξις*, &c.

'It must, I own, require a somewhat resolute patience to read the *Lectures through;* but I trust that, although parts may conveniently be omitted at first reading, the new work is so arranged, and *subdivided*, as to be quite easily accessible to any student who has a competent (not a profound) knowledge of former mathematics, and wishes to understand the subject. No new *notation*, for instance, is introduced—and after all, I do not employ many such —without a series of examples following, in numbered sub-articles, to render its meaning and its use familiar. And generally I hope that the progress is well *graduated* throughout; while I have *avoided* what may be called *talk*, with some care; and have only *one* metaphysical remark, in four and a half lines of a note!'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,
'*March* 6, 1862.

'. . . I have been so busy with your namesake defunct that I have had no time to write to your living self. I think I have fathomed him completely now; and a curious piece of the history of literature it will be when all put together . . .

'I have got hold of the syllogism for want of which the mediæval theology was all dogma and category, which made the people fight like *cat* and *dog*. I shall call it the syllogism of forbearance or of charity. A common syllogism makes A and B give the right to affirm C; but where is the argument of this kind?

I can affirm A,
I can*not* affirm B;
Therefore, I cannot affirm C.

Inferential non-affirmation. The laws of this syllogism are as follows:—The premises may be in any way a universal and a particular, provided that the *non-affirmed* be the particular. Or they may be both universal or both particular, provided the middle term have the same quantity in both. . . .

'How do the quaternions go on? Hoping there is nothing of sorrow or vexation to acccount for your long silence, I am,' &c.

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *March* 8, 1862.

[After reporting progress made in the printing of the *Elements*, and having mentioned that the second part of the Third Book would treat of the Differential Calculus of Quaternions, he proceeds]:—'As regards the Differential Calculus generally, I have lately been led to read again with care a good many works, your own included, at least in the parts bearing on *first principles*. When your letter arrived this morning, I was deep in Berkeley's "Defence of Freethinking in Mathematics"; the volume of his works, containing that Defence, &c., having just turned up. I think there is more than mere plausibility in the Bishop's criticisms on the remarks attached to the Second Lemma of the Second

Book of the Principia; and that it is very difficult to understand the *logic* by which Newton proposes to prove, that the *momentum* (as he calls it) of the *rectangle* (or product) AB is equal to $aB + bA$, if the *momenta* of the *sides* (or factors) A and B be denoted by a and b. His mode of getting rid of ab appeared to me long ago (I must confess it) to involve so much of *artifice*, as to deserve to be called *sophistical;* although I should not like to say so publicly. He subtracts, you know $(A - \frac{1}{2}a)\ (B - \frac{1}{2}b)$ from $(A + \frac{1}{2}a)\ (B + \frac{1}{2}b)$; whereby, of course, ab disappears in the result. But *by what right*, or for *what reason*, other than to give an unreal air of *simplicity* to the calculation, does he *prepare* the *products* thus? Might it not be argued similarly that the difference,

$$(A - \tfrac{1}{2}a)^3 + (A - \tfrac{1}{2}a)^3 = 3aA^2 + \tfrac{1}{4}a^3,$$

was the moment of A^3; And is it not a sufficient *indication* that the mode of procedure adopted is *not* the *fit* one for the subject, that it quite *masks* the notion of a *limit;* or rather has the appearance of treating that notion as foreign and irrelevant, notwithstanding all that had been said so well before, in the First Section of the First Book?

'Newton does not seem to have cared for being very consistent in his *philosophy*, if he could anyway get hold of *truth*, or what he considered to be such. In relation to *light*, he appears to have admitted *both rays and waves*, the latter being *excited* by the former. And I think that there is a real difficulty in reconciling the two parts of the Principia, above referred to. Indeed I have recently come to think that the "*momenta*," or "*incrementa vel decrementa momentanea*," of the Second Book (page 243 of the Third Edition), are just *infinitesimals in disguise;* although Sir Isaac did not like (it may be guessed) to employ the *word there*, after having so successfully avoided introducing the *thing*, or *thought*, in the First Section of the First Book. Even the "*motus, mutationes et fluxiones quantitatum*," in page 244 of the edition cited, were (I think) *infinitesimal quantities, as there used;* although—or partly *because*—the great author *adds, immediately afterwards*, "*vel finitæ quævis quantitates velocitatibus hisce proportionales.*" Yet I have quoted this very passage, in the manuscript of my concluding chapter, as at least *suggesting finite fluxions;* and have taken care to express my obligation, for other suggestions, to that splendid First Section,

above spoken of; especially for the suggestion of *magnified representations* of figures, the parts of which all *tend* to *vanish together*.

'For my part, I stick to the *finite quantities*, suggested in that page 244, as (in some sense) *representing the velocities*, or as being *proportional* to them. My library is poor in fluxional books, but I notice that Thomas Simpson treats *fluxions* as *finite*. (I have only a rather late edition, London, 1823.) Thomas Simpson's conceptions appear to have been very clear and distinct, and I do not venture to *say* that the geometrical investigation which he gives, of the *fluxion of a rectangle*, avowedly supplied to him by a young but unnamed friend, is *insufficient* in itself, but it fails to *convince me*, perhaps because I was not early *accustomed* to fluxions. Certainly there is no *neglecting* of ab, or $\dot{x}\dot{y}$, as *small;* for in fact that *rectangle of the fluxions* is not *represented at all* in his Figure, which is here annexed [diagram inserted] . . . He conceives the *varying rectangle* xy to be the *sum of two mixtilinear triangles*, of which the *two separate fluxions* are $y\dot{x}$ and $x\dot{y}$. This is very ingenious, but I do not feel sure to what degree I could *rely* on it and build upon it any superstructure, if I were now coming, for the first time, as a *learner*, to the subject. However, I suppose that a pupil, if reasonably modest, or even prudent, will take, *for a while*, his teacher's statements upon *trust;* reserving to himself to *return* upon them, and to examine closely their truth and logic when he shall have acquired some degree of familiarity with the subject taught.

'In a subsequent letter I shall perhaps be tempted to give you *my own proof*, resting on my own *definition* of *simultaneous differentials*, or *corresponding fluxions*, not only that the fluxion of a rectangle is as above, but *generally* that if $z = yx$ then $\dot{z} = y\dot{x} + \dot{y}x$, or $dz = y\,.\,dx + dy\,.\,x$, *exactly; even if* xyz, *and* $\dot{x}\dot{y}\dot{z}$, or dx, dy, dz, *be six finite quaternions.*'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,

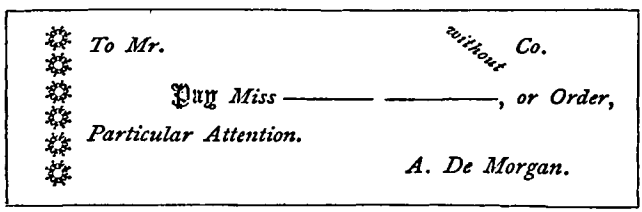
'*March* 11, 1862.

'. . . Newton was *shuffling*. He had (1712) to back out of infinitesimals, in order to unleibnitize his system. See accompanying tract, which read while the subject is on your mind.

'For myself, I am now fixed in the faith of the *subjective reality* of *infinitesimal quantity*. I intend to write on this subject when logic is off my mind, if that time should ever come. But *what* an infinitely small quantity is, I know no more than I know what a *straight line* is; but I know it *is;* and there I stop short. But I do not believe in *objectively realised* infinitesimals.

'Newton's $A+\frac{1}{2}a$, &c., always reminded me of an observer I once knew, who took to differential calculus. He was puzzled, ϕt being the velocity at time t, to get a velocity with which to go cleverly through dt. After much meditation on ϕt at the beginning, and $\phi(t+dt)$ at the end, he had recourse to his old tools, and *took the mean*. This made his mind quite easy. . . .

'I hope Lady H. and all the young people are well. Has Miss Helen forgiven my algebra yet? It was not so atrocious as the trick I played a young lady not many days since. She wanted my autograph, and I sent her this to stick in her book



She said she would not put it in the book, but would keep it to present when she knew how to fill it up.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *March* 14, 1862.

'. . . I enclose a *leaf* of my new book (pp. 391, 392), which is not yet signed for press. . . . You will see that I have entered on the printing of my remarks on the Differential Calculus of Quaternions; and have touched, in passing, on Fluxions and on Newton. (I read your pamphlet yesterday.)

'I wish that you would tell me whether you think that there is anything actually *wrong* in what I have said—though I dare say *you* would not have said it. At all events it is *true* subjectively, or *for myself*, since it has certainly been a study of the Principia,

without much attempt to understand the *momenta*, which has gradually led me to adopt *Finite Differentials*, as in my *Lectures* I already did. I know indeed that Cauchy came to treat dx and dy as finite, in at least some of his writings; but Todhunter (whom, notwithstanding, I admire) has evidently a holy horror of treating them as in any way *separate* quantities. Hutton and Saunderson seem to be quite content to get at results, by neglecting some fluxions as indefinitely small; for example, in the fluxion of a rectangle; but T. Simpson is far from being so *content*, whether he is always *clear to me* or not. Thus I find that at page 150, of the edition cited in a former letter, he says:—"but hitherto no particular notice has been taken of *the method of increments*, or *indefinitely little parts*, used (and mistaken) by *many* for *that of fluxions*, &c.," the *italics* being copied. About Berkeley, I am not unlikely to write again.'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,
'*March* 15, 1862.

'The word "*ultimate*" [*Elements*, p. 392] sets you right as to Newton personally. I am familiar with *finite differentials*. I think Lacroix, *inter alios* has h his differential of x, and the *second term* of $\phi(x+h)$ the differential of ϕx. How he hocus-pocuses it into $\frac{d\phi x}{dx}h$ I forget. But it is only an evasion of the *infinitesimal question*. If you admit finite quantities which are, by a *finito-facient* infinite multiplier, as the *ultima* or *nascentia*, you admit the *ultimate ratio* to have been arrived at *before* vanishing of terms —unless indeed you bring the pure 0 into finitude by a multiplier. It is quite true, no doubt, that the *limit* of $\frac{m\times\Delta y}{m\times\Delta x}$ is $\frac{WV}{PV}$ [referring to a diagram]. And if, with easier conscience, you get at $\phi'x$ by the limit of $\frac{m\Delta y}{m\Delta x}$ than by that of $\frac{\Delta y}{\Delta x}$, why, no doubt, you have a right to do it. But you may just as well take

$$\frac{k}{h} = \text{Limit of } \frac{\Delta y}{\Delta x}$$

at once; and then call h and k differentials of x and y—which they *are not*.

'But you have for you the authority of Legendre, who, when he has detected *in*commensurables, says, "But if there be no common measure, we may imagine a quantity which *serves them as a common measure* (*qui leur sert de mesure commune*). This will do in mathematics; it would not be a culinary process. Next time your cook leaves, *imagine* a person who serves you as cook, and see if your meat be not too much underdone.

'Take what plan you like, there is a road—a short cut—to limits, which sets you right.

> "For modes of nought let graceless zealots fight;
> He can't be wrong whose *limits* are but right."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *March* 18, 1862.

'I should have some things to object to in your last note, but am in a mood for giving *you* some new opportunities of criticism, by applying my own *Definition* of Simultaneous Differentials to an Example. But first let me protest that I conceive that I have a right, without becoming *ipso facto* absurd, to *propose a definition for differentials*, provided that I keep within bounds of *convenience* and of *analogy*, and do not violate received and useful *rules* of calculation. When you say, "But you may just as well take

$$\frac{k}{h} = \text{Limit of } \frac{\Delta y}{\Delta x}$$

at once; and then call h and k *differentials* of x and y—which they *are not*": I ask *on what grounds* do you *pronounce* so confidently that they are *not* differentials? Is it on the authority of Leibnitz? or from the etymology of the word? or do you appeal to general consent? The latter does not exist; and the former appear to me insufficient. I always thought it a trifling objection, which Lagrange made to the modern use of the word *limit*, in the beginning of the *Calcul des Fonctions*, that the *ancients* had conceived a limit to be a thing which could only be *approached to*, but *never reached*. Let me, however, guard myself, so far as to protest that I am far from seeking to exclude the notion of *infinity* from mathematics; I only say that *my* differentials—and I am not singular therein—are *not* infinitesimals. I *use infinites*, great *or*

small, in *getting at them;* but *they* are *finite.* As a little anecdote I may mention that I was asked, two or three years ago, by a clerical friend [*supra*, p. 116], to give him my opinion on a theological work of Dr. Mansel, especially as regarded the impossibility of a *Philosophy of the Infinite.* I replied, in substance, that "Philosophy" was a high and sacred word—

"How charming is divine Philosophy!" &c.—

but that I was sure that there existed a *Science of the Infinite,* to wit, *Mathematical Science.*

'Let me add, however, that *I* don't *adopt* the *definition,* that when $\frac{k}{h}$ = limit of $\frac{\Delta y}{\Delta x}$, then h and k are *differentials* of x and y; not that it does not seem to me quite competent to an elementary writer to do so, *if he pleases,* but because it *won't work* in *quaternions. I* must manage as well as I can, *without* assuming that $xy = yx$, or $xh = hx$, &c. Let then $y = x^2$, and let $\Delta x = h$: *my* calculus gives only, $\Delta y = xh + hx + h^2$, and *not,* generally, $= 2xh + h^2$. Consequently, my *quotient* of *differences* is

$$\frac{\Delta y}{\Delta x} = x + hxh^{-1} + h,$$

in which it is *by no means permitted* to change the *term,* hxh^{-1} to x, *in general.* Yet I *define* that the *part,* $xh + hx$, of the *difference* Δy, is the *differential* of y; and I *denote* it by dy, if h be denoted by dx; and this whether h be *small* or *large;* that is, in quaternions, whether what I call its *tensor* (answering to Argand's *modulus*) be a small or a large positive quantity . . .

'Your *opinion,* on any such subject, must have real *weight* with me, as with others; but I do not conceive that there exists *any authority,* by which the meaning of the *word* "*differential,*" has yet been *so fixed,* as to make it *improper* in a new writer to propose a *new,* or at least a *modified,* meaning of it. It has been, perhaps happily, too much a subject of dispute for *that.* Even the word *fluxion* has not always been used in one sense; and whatever Newton meant by it (which seems to me doubtful) in that page 244 of the Principia, some English writers have certainly treated fluxions as *infinitesimals,* and justified the omission of $\dot{x}^2$, in the fluxion of the square, on the ground of its *insignificance.* . . .

'I intended to *admit*, in a former letter, that Cauchy and *several* other French authors have considered that *differentials might be finite*,' although apparently *preferring* to deal with *differential coefficients*, of which you see that I can make no use. . . .'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,

'*March* 23, 1862.

'All your mathematics—all your use of a finite fraction $\frac{dy}{dx}$ — I can readily agree to. The sole question is the expediency of using the name *differential* for infinite multiple of a differential.

'You ask on what grounds I say that *h* and *k* are *not* differentials. Simply, because hitherto they *are not*. The true question is, on what grounds do I dispute the expediency of *your making* them take the name. Is it the authority of Leibnitz? No. The authority of an inventor extends no further than this—his terms have a right to respectful consideration; but the community, *quem penes arbitrium*, finally accepts or rejects. Mathematical language has a fermentation always going on which throws up all that does not assimilate. My belief is, that if you call *h* and *k* differentials, the community, when quaternions become *publici juris*, will *uncall* them. Is it etymology? So far as this. Etymology should not be offended against without good reason shown. And especially, when the proposed change is other than *extension*. Is it general consent? Yes. You affirm that no such consent exists. If by general consent you mean logically universal consent, I agree. No one term has that consent. But I believe, as a matter of fact, that there is a vast preponderance of writers who mean by a differential—when they use the word apart from "coefficient"—an infinitely small increment. This is a question of fact. If your reading denies the fact, I have nothing to say—mine affirms it.

'But are you forced upon the word "Differential"? They are —I mean *h* and *k*—the terms of Newton's *prime ratio*. I would rather use *primo-rationals* than differentials. But I dare say you may get a better word.

'These terms need a shaking very much. I should like to call

$\frac{dy}{dx}$ the "x rate of y," in abbreviation of the rate at which y is changing relatively to that at which x is changing.

'I think

$\frac{k}{h}$ prime ratio,
k the *prime* of y,
h the *prime* of x,

would give language that the common differential calculus might envy.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *March* 25, 1862.

'Thanks for your very interesting and partly controversial letter, which I received this morning, and which I hope to make soon the text, or at least the occasion, of another long letter from myself.

'I trust that you have received your "Mourey" [Mourey's Treatise on Negative Quantities, which had been lent by De Morgan], minus a paper *wing*, which may yet be found; also the O and P of my *Elements*. The baker [who took his letters to the post] is at the door.

'*March* 26.—You would be a most formidable opponent, in *any* controversy, but perhaps most of all in one which at all regards the history of the differential calculus. Yet I am somewhat surprised at your attributing a quasi-universality of usage to the interpretation of a differential as an *infinitesimal*, at least if you refer to *works* upon the *Differential Calculus*. I am quite aware that we are apt to *think* in *infinitesimals*, and that Laplace, for instance, continually *uses* them. You probably know *ten times* as many as I do, of such works; but really, at this moment, I cannot *remember one*, which *adopts* the interpretation in question; though *several*, perhaps *all*, *allude* to it, as the view of Leibnitz. *You mention it*, for instance, in pages 30 and 50, and, no doubt, in other places, of your Treatise on the Differential and Integral Calculus; although it seems clear that you *prefer*, as many others do, to use only *differential coefficients*; which will go a great way, I admit, but which, you know, *won't do at all* for *quaternions in general*, though

of course you *could* not notice that circumstance when writing, and are *not bound* to take cognizance of it now.

[Hamilton then cites as on his side Lacroix (*Elementary Treatise*), and Peacock, whom he proves to have considered it in 1816 as an *established fact* that *Analysts meant*, by the *word* "differential," the *first term* of the *development* of the *difference;* without the slightest hint that this term was to be *small*, or rather with the *express exclusion* of that condition. He adds, however, that Peacock was for excluding both infinitesimals and *limits*. 'In short, Peacock,' he says, 'was for relying on the method of the *Calcul des Fonctions*—which I greatly admired as a boy, but I believe that, when I was a young man, I had some small share in overthrowing (through $\epsilon^{-x^{-2}}$, see note to your page 176).' He next brings forward Cauchy (p. 18 of *Leçons sur le Calcul Différentiel*, Paris, 1829), making the statement, 'Quant à cette dernière quantité [dx], qui représente la différentielle de la variable indépendant, elle reste entièrement arbitraire; et on peut la supposer égale à une constante finie h, ou même la considérer comme une quantité infiniment petite.' He then speaks of Moigno as following Cauchy, and winds up as follows]:—

'In short, instead of fearing to be thought *singular* in doing this last thing, I fear rather that I may be supposed to *pretend* to *originality* in a course in which so many have preceded me. But you know that the *only* originality I pretend to consists in my having *combined* the *conception* of the *finite differential* with that of the quaternion; and so *constructed* a *calculus* which, imperfect as it still is, admits already of many useful applications, and has become an *instrument of research* in both *geometrical* and *physical* questions.

'If we were never to go beyond the *first order*, I think that *infinitesimals*, as they are very *natural*, might also perhaps content us. We can easily picture to ourselves little triangles, &c. But when we try to *imagine* an *infinitely small part* of an *infinitely small thing*, I think that we are apt to become *confused* in our conceptions; or at least we lose all that *facility* which the *first step* had *gained* for us. I know that it is possible to give *definitions* by which infinitesimals may have as respectable a *size* as any one can desire; but it is (as it appears to me) with *far greater violence to*

language than any which any use of the word *differential* can require. "*Infinite*" is a good old English phrase; is it more (*you* can tell, which I can't) than about *fifty years* since "*Differential*" was naturalised in our language? You know, of course, that Euler (in his *Institutiones Calculi Differentialis**) held that differentials, *and fluxions too*, were rigorously zeros.

'*March* 28.—You see that I am *in* for the *finite* differentials of quaternions; though, as you say, others may *uncall* them afterwards. After all, I am here only reproducing, in a more orderly manner, and I hope with greater clearness, a theory which was given in the *Lectures*.'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,

'*April* 1, 1862.

'I have received the Mourey. Never mind the cover. *Uno avulso non deficit alter*. I am thoroughly embusinessed for a few days—after which at you again.'

[Here follows a long letter from Hamilton, dated April 5th, containing a proof by quaternions of *Taylor's Theorem*, which is an anticipation of his full treatment of it to be found in the *Elements*, pp. 423 . . 432. The letter concludes with a P.S. in which he reverts, as follows, to the differential question:—

'It was stupid of me, when writing lately, to forget that *you use*, within square brackets, the symbol dx to denote the *differential* of x *in Leibnitz's sense*, as signifying an *infinitely small difference;* but I think that the current now runs the other way *in elementary books;* and that the *differential of* fx is now generally stated, in such, to be *equal* to the *second term of the development* of $f(x+dx)$, where dx is an *arbitrary increment;* although *some*, as in the Notes to Lacroix, cited in a former letter, *define* dfx to *be* that second term;

* Ticini, 1787.—The first passage that I refer to occurs near the foot of page lxii. in the *Praefatio*. But there are others in the body of the book. Euler's only *infinitesimals* were quantities *absolutely null;* whose *ratios*, however, were to be investigated.

while others give an *independent definition* of the *derived function* $f'x$ as the *limit of a ratio*, &c., and *then* write, as a *new definition*, the formula $dfx = f'x \,.\, dx$.'

De Morgan proceeds as follows in defence of his view:—

'*April* 7. . . . Now for differentials. The following is my *resumé* of belief and of fact:—

'1. Exclude fluxions. Fluxions were never anything but *velocities*. I believe Newton learnt the idea from the *intension* and *remission* of the schoolmen. You will soon receive a quotation from me on this point. The pure infinitesimals of Leibnitz mark all, I think, of the continental writers of note down to Lagrange, as Euler, Clairaut, D'Alembert, &c. Euler has a vagary about 0, but his dx is *not finite*.

'2. Lagrange heads a small school on whom you rely. Lacroix and Cauchy are its chief members. Peacock, when a young man and a translator of Lacroix, spoke of this school as "the Analysts." Lagrange used infinitesimals in the *Méc. Anal.* and elsewhere. All the physical writers do it, I think. All the mathematicians do it in conversation.

'Cauchy does it in several works, of which you have quoted one. In the last (Moigno) he relaxes and gives a choice. Moigno, p. 7. "La differentielle sera d'ailleurs, en général, une quantité finie ou infiniment petite, suivant que l'accroissement $\Delta x = dx$ sera lui-même fini ou infiniment petit." Looking over Cauchy's former works, I have no doubt the dog was thinking infinitesimals all the while he was making his poor young readers heave them up into finitude.

'3. There is a reaction against Lagrange. Duhamel, Navier, Cournot, are pure infinitesimalists. Some of them say an infinitely small quantity is one which may be made as small *as you please*. This is an evasion; but they do not mean that dx is *finite*.

'Lagrange dispensed with dx; Lacroix and Cauchy are your authorities. Against them I put all that I have stated.

'The integral symbol $\int ydx$ will be utterly divested of analogy to summation upon a finite dx. And this would be a sore blow to application.

'By-the-way, Poisson was a believer in the *reality* of infinitely small quantities—as I am. But it is late.

'P. S.—The Differential Calculus was introduced into Cambridge in 1816 . . . I shall seriously propose the "x rate of y." '

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 19, 1862.

'. . . I admit that in the symbol $\int ydx$ we naturally treat dx as an infinitesimal. About this, however, at least as bearing on quaternions, I may have something more to say.

'The Berkeleys which are here belong to the Earl of Dunraven, an old friend of mine, who lent them to me long ago, but wishes now to have them back again. Before I return them, is there anything that I could extract for you? In a pamphlet which you lately sent me, but which is not just now at my hand, I thought you seemed not to have seen the Bishop's "Reasons for not replying, &c." No doubt he was occasionally waggish, as in those "Reasons," in which I remember his remarking that perhaps "some zealous fluxionist" might think it worth while to answer Mr. Walton. But on the whole, I think that Berkeley persuaded *himself* that he was in earnest against Fluxions, especially of orders higher than the first, as well as against matter.

'I quite agree with you that it is an *evasion* to call things *infinitesimals*, which can only be *made* small.'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,
'*April* 20, 1862.

'. . . *Quoad* Berkeley, I have got him all. . . . I have no doubt Berkeley knew that the fluxions were sound enough. Remember that there had been three-quarters of a century of indivisibles, infinitesimals, &c., before Leibnitz published. Wallace alone wrote more of it than Newton and Leibnitz put together, but not of *calculus*, i. e. organised individual-into-species-collecting, rule-ending-in method.

'I sent you a Paper on Saturday. At the end you may form a guess where Newton first got the hint of fluxions. I do not mean from D'Oresme, but from some of the *intensionists*.'*

* *Supra*, pp. 353 and 580.

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *April* 22, 1862.

'Thanks for your letter received to-day and your Paper (on Probabilities, &c.) yesterday. The concluding extracts and remarks are very curious.

'I don't see why you should not succeed—if only, as you say, you could *provoke discussion*—in introducing your "x-rate of y," instead of what it is so tedious to write in the usual manner. But as I deal in quaternions, not with ultimate *ratios*, but with ultimate *laws*—not with rates of *variation*, but with *laws of growth*—or in more technical language, with *differential equations*, and *not* with *differential coefficients*, your new phraseology, good as it is, won't serve my purposes.

'I hope you understand me as admitting that any one who chooses *may* introduce infinitesimals into quaternions, just as freely as into what may be called (by contrast) *algebra*. In fact I find that my friend, Dr. Salmon, in an Appendix which he has lately let me see, though it is not yet published, to what will doubtless be a very valuable volume on Geometry of Three Dimensions, just takes the bull by the horns, and says that I *have* determined (for instance) the radial direction of a normal to a sphere by infinitesimals; and of course I *do think* habitually of *small triangles*, &c., in geometrical and physical *applications*.

'The only question is, *need we* regard *differentials* as other than *finite*? I know that Euler held them to be *rigorously zeros*; but I observe that Mr. Price, in a book on "Integral Calculus" (Oxford, 1854), which I thought that I had lost—it is called Vol. II. of a Treatise on "Infinitesimal Calculus"—takes up infinitesimals in the boldest manner.

'On the whole, I suppose you will be able to prove—or may *have* proved already—that you have the *weight* of modern authority on your side; but I still hold that the usage is not so *settled* as to make it *improper* for me to *define*, in my *own* book, as I am doing.'

From A. De Morgan *to* Sir W. R. Hamilton.

'41, Chalcot Villas, Adelaide Road, N.W.,
'*April* 24, 1862.

'Many thanks for Hart's Paper. I shall be very glad to keep it—as the author's autograph of the most elaborate and successful bit of rectilinearity I ever saw. The straight line is really a masterpiece of the nature of things. This reminds me of a joke I once heard: "Sir!" said somebody, "there is no straightforwardness about him; all he says is a parcel of fibs from one end to the other!" "No straight-forwardness!" was the answer. "Why! by your own account he lies evenly between his extreme points!"

'My objection, as you know, is to your calling your magnified terms *differentials*. All the rest I admit. But as you give permission to people to take infinitesimals if they like, they will do it.

'I think that people will not rouse themselves either to approve or disapprove of the "x-rate of y."

'I have nothing further to say about anything. I have just dispatched a big Paper on Logic to Cambridge, which I hope will be the last of a directly controversial character.

'Here is a bit of Latin which was given to me a few days ago:—

"'Αειδε ειδυλλιον, θεα,
Felis adest cum cithara,
Vacca lunam transilit,
Hoc jocoso motus visu
Rumpitur catellus risu,
Cum cochleari lanx abit."'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 2, 1862.

'After a hitch about money, the impression of my *Elements* has been resumed, and I send you (to wind up) a proof of the sheet signed this morning.

'It completes what I think useful to say, at present, about *differentials* of quaternions. You will see that I treat them (practically) as *infinitesimals*, when employed for purposes of *integration*.

What follows is far more original, but I fancy that you would not care for it.'

[In the middle of this summer De Morgan, though not changing his residence, had to change the local date of his letters. He thus heads a letter of June 4, 1862 :—

'91, Adelaide Road. (No more Chalcot Villas.)

'Says the Board of Works, says he, be it knowed
Your house is 91, Adelaide Road;
So paint it up where it may be showed;
Or I'll have you fined—if I don't, I'm blowed.'

On September 15 Hamilton begins a communication, occupying many letters, of his investigation of Röber's Construction of the Heptagon. I have already (*supra*, pp. 141 . . 147) given an account of Hamilton's work on this subject. Here therefore I confine myself to printing his introduction of it to his friend's consideration; but I add some incidental observations of De Morgan's which appear to me of interest. The final form of Hamilton's treatment of the problem is to be found in the *Philosophical Magazine* of February, 1864.]

From the SAME *to the* SAME.

'OBSERVATORY, *September* 15, 1862.

'Are you aware that it is possible with only *six* extractions of *square roots* (*two* being those required for chord and tangent of 36°) to approach so nearly to the cosine of the *seventh part* of four right angles, or to the positive root of the *cubic*,

$$2x^3 + x^2 - x = \tfrac{1}{4},$$

that the *sevenfold arc* resulting shall err (in defect) by only about *half a second* from the true and whole circumference? so that for *all practical purposes* a *regular heptagon* can be constructed by the right line and circle *alone*.

'My part in the matter is merely the having assigned, this morning, *how much* the construction *errs*. The *rule itself* is attributed by Röber—and with great plausibility, as it seems to me at

present—to the ancient Egyptians, who appear to have employed the heptagon in the architecture of some of their temples. The elder Röber was Professor of Architecture at Dresden about thirty years ago; and instead of claiming the construction as *his own*, professed (as I understand) to have divined it from a study of those ancient buildings. His son, Friedrich Röber, published it at Dresden in 1854, in a German Memoir on the Egyptian Temples, &c., which was sent me, with another on the Pyramids, from an unknown friend in Switzerland, through a son-in-law of the Archbishop of Dublin; so oddly do things come about.

'When I began to examine the construction, I expected to find some *considerable* error; and was *astounded* to find that I could not, with Taylor's Logarithms, decide to my own satisfaction whether the error, assumed of course to exist, was in *excess*, or in *defect*. The consequence was that I threw *tables* entirely aside (except, you may say, the *multiplication table*, which pound and mil will make at last the *only* one for common life); performed all operations by *arithmetic* alone, extractions being the chief, yet scarcely the chief trouble, such long though contracted multiplications were to be employed, when I aimed at 15 decimal places; and finally arrived at results of which I have given you the general outline . . .

'Will you tell me how you would find, by Horner's Method—which I once *knew*, but have never really *practised*—the root x of the cubic above mentioned? I made it this morning by a clumsy process,

$$x = \cos\frac{2\pi}{7} = 0{\cdot}62348\ 98018\ 5873\ 45.$$

You wrote to me, a good many years ago, on the subject; but I do not know just now where that letter of yours is'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *September* 18, 1862.

$1+3^2+3^4$

'I can believe anything of *six* square roots. But I cannot believe that the Egyptians employed them—though they may have hit on a method which requires six square roots to represent it arithmetically.

'I send you every figure of my first attempt at the solution of

the equations—done just as my pupils are made to do it. Horner himself did not hit the extreme of organisation. He and Young—his earliest expositor—both got the places to be thrown away in contraction, by mother-wit and look-out. The process I send you is purely mechanical, and done as a skeleton from beginning to end. You will find my scheme in an Appendix to my Arithmetic.

'As to the rest, meaning everything in existence except Horner's Method, I have nothing particular to say. I have some remembrance of a good approximation to a heptagon in the common books of mensuration. But, after all, nothing is so short, even for most accurate work, as pure trial with the compasses. With *hair*-compasses, I would beat all the methods, in time and accuracy both. Do you know the *hair*-compasses? . . . I should like to see the formula of the six square roots.

'*September* 23.—You have got the principle of Horner's Method . . . The table of Pitiscus is his corrected Rheticus—the Thesaurus—for which see my article on Tables. It is *rarissimus*. I have Cavendish's copy. The Duke of Devonshire sold all Cavendish's books—which looks odd. But all the C.'s hate their great chemical relation—and with good reason. He was a heartless brute. The original Rheticus, the Opus Palatinum, will do well enough; it is not quite so rare. Or Briggs' *Arithmetica Britannica*. Take notice that Horner's Method is not algebra. It is pure arithmetical organon—the true extension of division and the square-root. Whether Horner's immediate predecessor is a Sanscrit, or a Tartar, or an antediluvian, nobody knows.

'I am surprised that the Egyptians should have got so close a method—and I admire Röber's ingenuity. But I am no way surprised at six square roots giving 15 places . . . Seeing that my last figures differed from yours, I repeated the calculation, and find your figures are right *ab ovo* (.) *usque ad mala* (*mala*—apples of discord, the last figures of a calculation). I also verified it by old Pitiscus as in accompanying Paper.'

'*September* 29.—[Replying to Hamilton writing 'I should like to know exactly *what you claim*, in relation to Horner's Method. Is it more than the *arrangement* of the figures, which, however, I hold to be of *vast* importance in a long calculation?'] You evidently like calculation, in spite of sky-high quaternions, &c. If you had not the organ in your head, not all the quaternions could give it.

I take it that you and I are the only persons who know $\frac{2\pi}{7}$ to 22 places.

'As to what I claim, I know not *how much* it is; but I know *what* it is. I know that I bridged the chasm—how many arches it took I do not know—which separated Horner and his followers from seeing that his method *is* the natural and proper development of common arithmetic; so that division and the square-root are true cases of the whole method. Horner himself did not see his own fame; did not know that he was the first European who had gone beyond the nameless Brahmin, Tartar, antediluvian—whichever he was—who constructed the rule for the square-root. He had high notions, and looked upon his method—very justly—as a particular case of one which would solve, say $x^{\sin x} + (1-x^2)^{\tan^{-1}x} = 1$, if there be a real solution, on which I do not pronounce.

'My history of the problem is in the *Companion to the Almanac* for 1839. The article on *Involution* in the *English Cyclopædia* unites two articles of the *Penny Cyclopædia*. Part 16 of the Division of *Arts and Sciences* contained all the letter I; and I find I may say "pars magna fui." Witness: Incommensurables, Infinity, Involution, Integration (common and definite), Invention and Discovery, Irrational quantities (the only modern account of the 10th Book of Euclid), Interest.

'P.S.—I know all about Horner but his *name*. He is *W. G.* Horner—but I never saw it stated what W and G stand for.

'*November* 16.—[Referring to a proposition suggested by a mode of tracing an hyperbola communicated by a Pennsylvanian visitor, he writes]:—'I do not remember the *class* of propositions to which this belongs. I care no more for an isolated theorem now, in these things, than for 22 + 13 = 35, or any other case of addition. What is the connexion of all this with higher generalisations? And as I attend to it only once a year, when teaching comes on, I always write to Ireland for a wrinkle about conics or quarics (—I will not say quadrics—quarrée, carré—I am not sure I will not stand up for *carrics* or *carics*) when I want one. It is "Please remember the grotto—only once a year"—whether you understand the allusion or not.

'I am excogitating the subject of infinity—with a view of writing upon it. There is a question I put to every thinking man

who comes in my way—*I have various answers.* Consider space as objectively external—merely for convenience. Does the *totum spatium*—the universe of space—contain twice as many pints as quarts? Yes or no. No refinements about inconceivabilities, &c., &c. I think the idea of measuring out infinite space in quart measures a very grand one. For myself—after trying for years to evade it psychologically, ontologically, pseudologically, and amosgepotically—I find no rest for the sole of my foot except upon the assertion—which I now fully believe—that the universe *does* contain twice as many pints as quarts. That is, if worth 2*d.* a quart, it is worth 1*d.* a pint. You surely cannot deny this—and how can it be unless there be twice as many pints as quarts?

'*Amosgepotically*—ἀμωσγεπως—somehow or other. I have got this word to express my way of explaining things. If I am asked—as an omniscient philosopher—for the *why* of anything—and I say I don't know, I am despised—if I say it happens somehow or other, I am looked upon as a very common sort of thinker; but if I say it happens *amosgepotically*, I at once take rank with the high aristocracy.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *April* 13, 1863.

'Are you gone to the moon, or to Java, or have you eloped with a mermaid? These are three of the possible questions, and are put as specimens; the whole list is too long. I should like to know among other things when the quaternions will be finished...

'I have been jotting down my ideas about infinity; that is quantitative infinity, not the absolute, or the unconditioned, or zero passing into being, or any of the Nicotian* views of the subject. I want to test the question whether mathematicians do or do not actually admit an *infinitely small* quantity, by premises from which it may be actually squeezed, as therein contained. If I say that $\overline{A\ B}$ is an infinitely small part of a line, I make it so, and I cannot be said to have *deduced* it. I may say, it shall be any finite line, however small, but I will not, by assumption, introduce any line except what is between $\frac{m}{n}$ and $\frac{m+1}{n}$ of the whole,

* *Supra*, p. 446.

where m and n are two finite numbers. Certainly a person who takes this ground may avoid being forced to acknowledge an infinitesimal.

'But now, take him this way. He admits the concept, a straight line. He admits the concept, a point. He acknowledges that the straight line contains an infinite number of points; that is, more than any finite number which can be named. This is the common ground of all mathematicians. He then hands me over a *genus*, namely, that of which the individual is a point in a given straight line. From this genus I have a right to select a species by any difference I please. I select by this difference—"dividing the whole into two commensurable parts"—say each such point is a C. He grants me all the C's, because he gave them as individuals of an acknowledged genus. He will grant me that no part of the straight line has all its points among the C's; that is, he grants an *interval of length* between every two C's. I come at these intervals of length as necessary results of his own data and admissions. Now, all the C's being taken, that is, selected from his *genus*, what are these intervals? Are they finite? Certainly not; they are then infinitely small.

'Here are points which approach without limit, and do not ultimately coincide. Or rather, without any subsequently ordained approach, here are points which do not coincide, but the distance between which is less than any finite quantity which can be named. Not laid down for that purpose and upon that hypothesis, nor increased *ad infinitum* by the proponent, but selected from a greater infinite acknowledged by all, and handed to him by common consent.

'It appears to me that there is no getting out of the *concept*—concept without *image*—of an infinitesimal, as actually necessary to what all admit.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 16, 1863.

'. . . I remember your proposing to me a question on the *Cubature of Space*, to which I thought of replying by a question respecting the *Quadrature* of the *Infinitely Distant Surface*. Fine fun there will be for the Hamiltonians—I do not mean myself—when you come out with your *pints and quarts of infinity!*

'But it appears to me a most legitimate and interesting subject of psychological and literary inquiry, what do *mathematicians mean* when they talk of *infinites and infinitesimals?*

'Has it ever struck yourself—as it does me—that you *believe* more in infinites now than when you wrote your "Differential and Integral Calculus"? I catch myself *believing* occasionally in the *line* at *infinity* in a *given plane*, and what is far more odd (yet useful), the *plane* and *circle at infinity* in *space*. Then I *look down on myself!*—but not on *you*.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *April* 26, 1863.

'I do not believe in infinity *more* than I did in my "Differential Calculus"; but I certainly had not so decided a belief, only a leaning. The fact—which I shall promulgate—is this. When I was a boy at Cambridge, I observed that the first thing every high undergraduate, and every mature B.A. did, was to settle definitely and irrevocably the true foundation of the Diff. Calc. I saw this, and made up my mind that they *ought* to wait, and that I *would* wait. All these years—say 38—I have pondered the subject, with a resolution never to commit myself on the wrong side of *fifty*. And now, having with a very unbiassed and very long meditation on the subject made myself sure that infinity is a subjective reality, I intend that it shall be so for the future. But I leave all other minds to stand on their own bases. I do not assume that it is a subjective reality in another subject.

'But I do not say that two parallel straight lines meet at an infinite distance. They meet *never*. And I distinguish $\frac{1}{0}$ from all the lower infinites, by the same distinction which exists been 0 and the infinitesimals.

'I am seriously of opinion that we should constantly flounder and get wrong in using *limits*, if we had not the light of infinity to guide us.'

From the Same *to the* Same.

91, Adelaide Road, *July* 31, 1863.

'. . . I have got out of logic for the moment—I suppose you received a paper from me—and am now thinking seriously of my attack on *infinity*.

'But, first, I am collecting all my queer books, circle-squaring, earth-fixing, &c., &c., &c., and making short notices. It is a wonderful sight, when many of these things come together. I should not like to meet the whole set in conclave assembled. I should recall the apothegm attributed to Whewell, when he came up, quite fresh and northern, to Cambridge. Watching a herd of swine going along the street, he remarked—"They're a hard thing to drive—very—when there's many of 'em—is a pig."

'You will make a very big book of the new quaternions. Is this your elementary summary? You or some one else must do this.'

[In October, 1863, Hamilton refers to De Morgan the method of contracted multiplication devised by Mr. Boyers, a self-taught arithmetician in the employment of Mr. J. G. Rathborne of Dublin. It has been mentioned *supra*, p. 152. De Morgan's comments upon it are of interest.]

From the SAME *to the* SAME.

'91, ADELAIDE ROAD, *October* 21, 1863.

'I have seen the attempt you point out in various places, and under various modifications. I can hardly say where. (Folio, London, 1750.) "A method to multiply or divide any number of figures by a like or a less number, so expeditely, that any fifty figures may either be multiplied or divided by any fifty figures, all in one line, in five minutes' time . . . " "Invented by Quin Mackenzie-Quin, Esq., at the eighth year of his age." The "five minutes' time" is all nonsense—but the principle is something like that you speak of.

'The thing saved is *writing*, and nothing else. All the operations are those which must be done in any case. Now, writing is the least of all the troubles, when the pen is in hand. And the carrying so many figures in the head is a perplexity. Anyone who could do the short method with ease could do the common method much more easily. I mean, both as to time and trouble. Nothing has ever come into use in arithmetic which combines two really distinct operations in one, except when the second operation has *no new fag*, as in doing the multiplication and subtraction in one, in dividing.

'I hold that nothing can be with profit a single operation which is anything but one of those in the first Appendix of my Arithmetic. Laborious and rapid calculators there are many, who know the method—for anyone could invent it—but they know from experience that such abbreviations do not pay. The risk of error is very great.

'The lowest operations of arithmetic must be *painless*. If anyone is so quick that $ad' + bc' + cb' + da'$ is a painless operation, he may be safe in using the short method.

'A person in love with a short method—the inventor, for instance—puts on all the steam, and gets power by practice. But what would he have done with the common methods if he had put on as much energy to them? When old Mr. Weller was taunted with the speed of the railway, he retorted, "What rate do you think I could have kept a coach going at for £100,000 a mile, paid in advance?"'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 23, 1863.

'There is great sense in what you say about the method of contracted multiplication which had been sent to me; in the very example selected, the inventor had made several mistakes, after the first few lines. I think that *you* and *I* could *use* the method safely, precisely because we don't want it. Still it was *new* to *me*, as a *method* of *writing* (and arranging) the steps of a long multiplication; . . .'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *October* 10, 1863.

'I have found, by accident, perhaps the last version of the rule of multiplication in existence. It is by Lieutenant-Colonel Oakes of the Indian Army; and is in the "Assurance Magazine" for January, 1863. He takes the multiplier and reverses it on a bit of card, which is really essential when the figures are many. Thus, in an example, he proceeds as on the other side. But I have seen it several times before: never with the reversal, I think. Perhaps the rule is not to be despised when neither factor exceeds three figures.

'P. S.—There are no more processes in [an example of the common method] than in the *short* way. And the head-work of the addition is petrifaction and worse. I would rather be a skeleton in the drift, and hear philosophers swearing I was two million years old, when I knew I was not two thousand, than have much to do with it.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *October* 24, 1863.

Returning to the subject of Horner's Method, Hamilton asks De Morgan:—'Should you consider a schoolboy unusually dull, who could not be taught to practise this contracted method safely?

'As to Horner's Method, generally, it might perhaps be described, in arithmetics, as *Complex Evolution;* and a good preparation for it might be the Rule of *Complex Involution* corresponding; namely, one for calculating $x^3 + 3x^2$, &c., or even x^3 itself.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *October* 28, 1863.

'. . . I find the boys in my classes take Horner very well, contraction and all.

'Peter Nicholson called it *E*volution, and I followed him. According to him, the name Involution is extended from

$$(((x+0)\,x+0)\,x+0)\,x+0\ldots$$

to

$$(((x+a)\,x+b)\,x+c)\,x+d\ldots$$

and Evolution is its inverse.

'But in teaching calculation, it is only those who have the spirit to give, who can give it. It will not go through unworthy vessels, like the Apostolic succession. A calculator who meets his class with a cut-and-dry example, prepared by himself *beforehand*, will not make his followers believe in themselves: how should they, when he does not believe in *him*self? He must invent the example before their eyes, throw himself into it, as he would have them do; and make his mistakes, as he knows they will do.

'So no more, it being time to go to roost and leave roots.

'*November* 8. . . . I have had a letter from an X. Y. Z., who assures me that, if I do not attend to it, my reputation will suffer. It is James Smith's diagram, method, conclusion, and style; and I have no doubt that J. S. is the man. He says, alluding no doubt to you, that some calculations would make it seem that the 20-sided (he says 24-sided) polygon is more than 3·125 times the diameter, which is a very curious paradox and "easily explainable." But he does not condescend to explain. Well! If I go, you go too—that's one comfort. What a queer lot I have to deal with! One fellow, who signs ⟨o⟩, sends an interpretation of 666 twice a-week. *Ex. gr.* cabbala—χαββαλλ, and chemistry—χιμεια—as he will have it spelt. He begs me to remember, when the millennium comes—soon—that he told me. I take him, from the symbol, to be the round man in the three-cornered hole, if you know the joke.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 10, 1863.

'. . . Have you read even the early part of Cotes's *Harmonia Mensurarum*? It is really a work of genius, and I quite agree with its author, in considering Ratio* as a *Quantity sui generis* of which a Logarithm is the *Numeral Measure*. ("Logarithmi sunt rationum mensuræ Numerales.") A *duplicate ratio* is really, in his view, a *double ratio*, or has a *double measure*, in any *System of Measures*, which latter *may* be *quantities*† *of any sort*. ("Hæ mensuræ sunt quantitates cujuscunque generis, quarum magnitudines magnitudinibus rationum sunt analogæ.") The Modulus of any *System* is the arbitrarily *adopted measure* in *that* system of the fixed *Modular Ratio* of $1 + \frac{1}{1} + \frac{1}{2} + \frac{1}{6} + \frac{1}{24}$ + &c., to 1; that is, of e to 1, if $\int_1^e \frac{dx}{x} = 1$; for though Cotes does not use this *notation of definite*

* I have long since studied and admired the fifth Book of Euclid. But the *timidity* of Euclid in dealing with *Ratio* as a *quantity*—which he never expressly does that I know of—delayed the discovery of *Logarithms* by almost two thousand years.

† At least any quantities which admit of *contrary affections*. ("Rationes quæ dicuntur majoris et minoris inequalitatis contrarias habent magnitudinum suarum affectiones.")

integrals, he employs the *things themselves*. In several applications, his *modulus* M is a *line;* for example, in his rectification of the Apollonian *parabola* it is the distance AF from vertex to focus. In other applications, it is an *area;* for instance, in one of his *quadratures* of the *hyperbola* it is a *parallelogram*, and in another it is a *triangle*. In *Briggs's System*, or Canon, Cotes says that it is 0,434294481903, &c. He makes *no mention of Napier!* At least I have not been able to *find* any such mention, in the places where I should have *expected* to meet it, either in the "Harmonia" &c. *itself*, or in such *notes* by the editor (Robert Smith) as I have looked into. Is not this very extraordinary? Was it because Napier was a *Scotchman*? Can they have hoped that *Briggs* would be remembered, and *Napier* forgotten? I shall really be much obliged by your correcting me, if *you* have observed any reference to, or mention of, the true discoverer. My copy is dated "Cantabrigiæ, MDCCXXII."

'Of course the *modulus* of the *natural* (or Naperian) logarithms is *positive unity;* for it might be a *negative number*, in some other system. Cotes uses the notation $M\left|\frac{b}{a}\right.$, to denote the *measure*, to the (numeral, or linear, or superficial) *modulus* M ("pro modulo . . .") of the *ratio* which the *magnitude b* bears to the magnitude *a* of the *same kind*. Many of his formulæ are therefore *translated* into more modern ones, by simply reading $\left|\frac{b}{a} = \log.\frac{b}{a}\right.$; where the log. is the *natural* one.

'An ANGLE, liks a *ratio*, may be *variously measured;* but Cotes appears to consider (and R. Smith expressly calls) *that* well-known *angle*, which an *arc* equal in length to the *radius* of a circle subtends at the *centre*, the MODULAR ANGLE; and the *measure of this angle*, whatever it may be in any particular *System of Measures of Angles*, is called the MODULUS *of that System*.

'If, for instance, angles be measured by sexagesimal *degrees*, or if the *Measure of an Angular Degree* be *assumed* to be *Unity*, then *in that System*, the *Modulus* is 57·2957795130, &c. (I copy these figures without examining them.) The *modulus* of our *usual theoretical system* of *angular measures* is *unity*.

'Cotes's HARMONY *of Measures* appears to consist chiefly in this.

He seems to have perceived, though not in our notations, that

$$d \,.\, a \tan^{-1}\frac{x}{a} = \frac{a^2 dx}{x^2 + a^2}, \text{ and } d \,.\, b \log \frac{x+b}{\sqrt{(x^2 - b^2)}} = \frac{-b^2 dx}{x^2 - b^2}.$$

Making, then, $b = ia$ $(i = \sqrt{-1})$, we have

$$d \,.\, ia \log \frac{x+ia}{\sqrt{(x^2+a^2)}} = d \,.\, a \tan^{-1}\frac{x}{a} :$$

in fact, if $x = a \tan v$, and *if we admit* that $\log (\cos v - i \sin v) = -iv$, we have thus

$$ia \log \frac{x+ia}{\sqrt{(x^2+a^2)}} = av + ia \log i = av + \text{const.}$$

(I am aware that a *different constant* might be taken here, but this is immaterial, when only the *indefinite integral* is wanted.)

'Hence, when in his *Tables of Forms of Fluents** he arrives at a term or factor of the form $R \left| \frac{R+T}{S} \right.$, where R is a constant given by its square, T is a variable, and $S^2 = T^2 - R^2$, R being at first supposed to be *real*, he directs his readers to *substitute* in the *case* $R^2 < 0$, instead of what we should write as

$$R \log \frac{R+T}{\sqrt{(T^2 - R^2)}}, \text{ what is in our notations, } \sqrt{-R^2} \tan^{-1} \frac{T}{\sqrt{-R^2}};$$

or what he calls the *measure of this last angle*, to the *modulus* $\sqrt{-R^2}$.

'Cotes drops a *remark* in passing (p. 28), from which it appears —as other parts of his writings also indicate—that he was *familiar* with the theorem which we write as $\varepsilon^{i\theta} = \cos\theta + i \sin\theta$; or as $\log (\cos\theta + i \sin\theta) = i\theta$, in the most elementary form of this last *logarithm*; although I think that "*ducta in* $\sqrt{-1}$" ought to be "divisa per $\sqrt{-1}$," in the particular passage referred to. In the next page he refers to Cardan's Rule, &c., as showing: "qualis sit ordo Naturæ transeuntis ad Anguli trisectionem a trisectione Rationis."

* Or perhaps, rather, *Tables of Fluents for certain Forms of Fluxions*. Have these been *all absorbed* in *recent* Tables?

'He seems—and so does his editor, R. Smith—to use *Fluxions* as *Infinitesimals always;* at least I have not noticed an exception.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *November* 14, 1863.

'I knew the *Harmonia Mensurarum* many years ago, and I have looked again at the beginning. I do not think Cotes meant to suppress Napier; his "logarithmus *Briggianus*" is used as opposed to *something else*, and that he knew was Napier's in the mind of all his readers. He was very young, and young men are not historians. Perhaps he had never seen Napier's works. You may not be aware that Napier did not use what *we* call Naperian logs. He had a modification. His log $\frac{m}{n}$ is our $10 + \log n - \log m$ (Naperian). Moreover, he gave only sines, &c. Speidell (1619) first published our *Naperian logs* of numbers in which $\text{No} = \epsilon^{\text{Log}}$. Cotes knew logs, probably, only through Vlacq's version of Briggs.

'I object to Halley's and Cotes's *mensura rationis*. *Mensura* is not the word. I take it that ratio is a *quantity per se*, of which common number and fraction are the simplest cases. But I cannot *double* the ratio of 4 to 3 into anything but that of 8 to 3. I admit that the operation $\times \frac{4}{3}$ done twice is $\times \frac{16}{9}$, and the λογων ἀριθμος is a very happy idea. But I cannot hold that the number of operations is a *measure* of the work done, when each operation works upon all the preceding work. Let ϕx be a function of x, and let $\phi\,(\phi x)$ be $\phi^2 x$, &c. We may proceed by jumps from 0 to ∞ through $x\ \phi x\ \phi^2 x\ \phi^3 x\ \ldots$ If we extend our notions, and invent $\phi^{\frac{m}{n}} x$, we may proceed continuously by continuous variation of $\frac{m}{n}$; but $\frac{m}{n}$ is not in any sense a measure of the *value* of $\phi^{\frac{m}{n}} x$. If $\phi x = ax$, we have the system x, ax, $a^2 x$, &c., which, making $x = 1$, after all is done, gives us the progression of ratios. Here, in $\phi^{\frac{m}{n}} x$, $\frac{m}{n}$ is the φ-ων ἀριθμος, but not the φ-ου μετρον. Measure and measuree go rateably together, in every use of the word except only the—therefore—objectionable *mensura rationis*. . . .

'Cotes was an infinitesimalist; so was Newton, till he abjured in 1706. But he continued a momentarian—which I take to be a $(dx)^{\infty}$ -man—all his life.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *November* 17, 1863.

'Thanks for your remarks on Cotes, especially in relation to Napier. It is pleasant to see grounds for acquitting the former of any disrespect to the latter. I have been long aware, or at least it has been my impression, that Napier did not *treat* his *logarithms* as *exponents* of powers of a *base* ε; and I do not know—but, doubtless, *you do*—whether he ever had occasion to compute the number 2·718281828... *at all.*

'About thirty years ago, I read a part of the "Canon Mirificus," or of the Introductory Remarks, in the Library of T. C. D.; and there remains on my mind an *impression*, that Napier's process was *analogous* to the integration of the *differential equation* $dy = ydx$; whereas Cotes virtually calculates the *definite integral* $x = \int_1^y \frac{dy}{y}$. I think also that Napier computed rather logarithms of *cosecants* than of *sines;* and remember that he seems to have a distinct *conception* of *flowing quantity* . . .

'I am not a particular admirer of the *phrase*, "Mensura Rationis;" but do not agree with you—who are, however, in the immense majority at present—in *identifying* "Ratio" with "Quotient." To me it appears to be of importance to *distinguish* between these two *conceptions*—for I take them to be *two*; and that there would be an advantage in not using *indiscriminately* the *two names:* although I grant that modern custom is against me, and often carries myself with it on this point. Speaking carefully, I should say:

Yard is *to* Foot, *as* Three to One;
Yard divided *by* Foot *equals* Three.

'*Duplicate Ratio* (λόγος διπλάσιος) was used, I grant, by Euclid in a *technical sense;* but do you think that he would have admitted, in *any* sense, that the *ratio* of a fathom to a foot was *double* of the ratio of a yard to a foot? In such *phrases*, still preserved, as *sesquiplicate ratio, one germ* of *logarithms* may be seen; or say, in

triplicate ratio (λόγος τριπλάσιος), if sesquiplicate be a later term: the *other germ*, in ancient science, being (I think) the plan of Archimedes, for *expressing large numbers*, and for *multiplying* them together, as what we should now call powers of ten.

'If the *phrase*, "triplicate ratio," be retained, it seems to me that we may in consistency speak of *dividing* the *ratio* of sphere to sphere, *by* the ratio of diameter to diameter, and obtaining as the *quotient* the *number three*. *Logarithms* would generally thus be

Quotients of Ratios.

But it is important to observe that *such quotients* admit of *affections*, as positive or negative, which the quotients of ordinary *magnitudes* do *not*. No length divided by a length gives any quotient but a *number*, commensurable or incommensurable, but always *positive*, or more properly *signless*: whereas I should say, following out the foregoing view that the quotient,

$$\frac{\text{Ratio of yard to foot}}{\text{Ratio of foot to yard}} = -1.$$

Negatives might thus have been anticipated, as I conceive, if *quotients of ratios* had been studied: a ratio of *major inequality* being exactly *as much the opposite* of the (*inverse*) ratio of *minor* inequality, as the *step* BA is the *opposite* of the step AB, and gives the quotient,

$$\frac{BA}{AB} = -1.$$

'In fact, I am disposed to *define*

"Negative One,"

as being thus the *quotient* of a *step* (or of an *interval*—and a *ratio* may be regarded as a *sort* of *interval*—), divided *by its own opposite* (or *inverse*); although it then requires some additional consideration to justify the *phrase* "Minus One," or the *notation* -1, so as to show its consistency with the earlier use of the sign $-$ in subtraction.

'"*Quotient*," at least when expressed by a *positive whole number*, or by such with a *fraction* (although quotients *include*, as above, not only incommensurables but *negatives*), appears to me to be a simpler or *earlier conception* than that of *ratio*; because it may be derived from the comparison of *two magnitudes*; whereas ratio is scarcely intelligible without *proportion*, which latter requires the comparison of *two pairs* of magnitudes.

'When a carpenter applies his foot-rule to measure the length of a room, and finds it to be *twenty feet*, he notes (perhaps) the *number* 20—or if there be six inches over he need not be a very learned carpenter to write down $20\frac{1}{2}$, and may thus be said to have *divided* the length *by* the foot and obtained 20 or $20\frac{1}{2}$, as the *quotient*, without having yet formed the comparatively abstract notion, that the length of the *room* bears the *same ratio* to his pocket *rule*, as the length of a *hall*, three times as long as the room, would bear to a yard measure.

'On the other hand, a very young geometer may see, what a common carpenter might find too abstract a conception, that if there be two right-angled isosceles triangles, hypotenuse bears to hypotenuse the *same ratio* as base to base; or (alternando), that the hypotenuse is to the base in *one* triangle as hypotenuse to base in the *other;* or (invertendo), that base is to hypotenuse in the first as base to hypotenuse in the second; and this, *without* his having yet *thought* of *measuring* either line by the other, or of arriving at the *incommensurable quotient*,

$$\frac{\text{hypotenuse}}{\text{base}} = \sqrt{2}.$$

'Indeed it does not appear to me clear that Euclid, familiar as he was with what I regard as the *subtler conception* of *ratio*, ever formed, or at least used, the *notion* of *such* a *quotient* at all; although, it may be *suggested*, or at all events *simpler quotients are*, as above, by the rudest and most practical acts of mensuration, or rather by *every* measurement: and though Euclid embodied, in his *definition of proportion*, the thought of inquiring *how often* a lesser magnitude is *contained* in a greater of the same kind, that is to say, *how often* (quot) can the one be *subtracted* from the other, *with or without u remainder*.

'I quite agree with you that in such an expression as $\phi^{\frac{m}{n}}x$, $\frac{m}{n}$ is *not* the $\phi x - ου\ μετρον$; and am content that you grant it to be the $\phi - ων\ ἀριθμος$. The *exponent* $\frac{m}{n}$ is *no measure* of the *result* of the operation $\phi^{\frac{m}{n}}$ on x; but I think that it *is*, in an intelligible sense, a *measure* of the *operation itself*.

'*Centupling* is, or may be considered as being, an operation compounded of two *decuplings*, the *second* of which, *as an operation*, is exactly similar and *equal* to the *first;* and therefore it seems to me not improper to *say*, that the *operation* "centupling" is *double* of the operation "decupling;" or for a like reason, to *retain the ancient expression*, that the "*ratio* of a hundred to one" is *duplicate* (or double) of the "ratio of ten to one:" and similarly in other cases.

'As an additional illustration of what I *mean* by *quotients*, I may observe that the equation

$$\int_a^b \frac{dx}{x} = \log \frac{b}{a}$$

would be said by me to hold good, for *any two given homogeneous magnitudes*, a and b; x being a *third* but *variable magnitude*, of the *same kind*, which is here supposed to receive *infinitesimal increments* (or decrements*) dx, while changing gradually from the *given lesser* (or greater) magnitude a to the *given greater* (or lesser) magnitude b; and all this *without the necessary selection of any unit of magnitude*. For instance, let a = a *pound* avoirdupois—not any *number*, but simply a pound weight—and let b = two pounds. As an approximation, let dx = an *ounce*, throughout; then the equation becomes

$$\frac{\text{one ounce}}{\text{one pound}} + \frac{\text{one ounce}}{\text{one pound one ounce}} + \&c. + \frac{\text{one ounce}}{\text{one pound fifteen ounces}} = (\text{nearly}) \log \frac{\text{two pounds}}{\text{one pound}};$$

each *dividend*, and each *divisor* being here a *weight*, but the *quotients* being all abstract *numbers*. Of course, this *comes*, in *calculation*, to saying that

$$\tfrac{1}{16} + \tfrac{1}{17} + \tfrac{1}{18} + \&c. + \tfrac{1}{31} = (\text{nearly}) \log 2;$$

but I think that the *conception* is not quite the same. It is only, you know, for *products*, &c., that we require a previous *unit;* and accordingly, while I regard, as above, the *quotient of two ratios* as a

* Of course a *decrement* of a magnitude, divided by the magnitude *itself*, would be considered to give a *negative quotient*.

logarithm, namely, as the *natural logarithm* of n, if the *dividend ratio* be that of n to 1, and the *divisor ratio* that of ε to 1; or generally as the logarithm of n, in the *system* of which the base is m, if the dividend ratio be still that of n to 1, but the divisor ratio that of m to 1, I cannot *definitely interpret* a *product of two ratios* without a previous selection of some

Unit of Ratio;

the *natural unit* being evidently that which Cotes calls the Modular Ratio, namely, that of ε to 1. *With this selection* the *ratio* of n to 1 would be *represented* in *calculation* by the *natural logarithm* of n, just as a foot is represented by 12, if an inch be taken for the unit of length; and the *product of two ratios* is then simply the *product of the natural logs* corresponding, or the *ratio* which *corresponds* to *this* product considered as a natural logarithm; while the *quotient* of two ratios *retains* the *same value as before*. In short we should thus have the formula,

$$(\text{Ratio of } m \text{ to } 1) \times (\text{Ratio of } n \text{ to } 1) = (\text{Ratio of } p \text{ to } 1),$$
$$\text{if } \log m \times \log n = \log p.$$

'If it be true, as I think, that the *Conception of Ratio* is a somewhat *more subtle* one than that of *Quotient*, it may be expected that the conception of a *Ratio of Ratios* will be found to be still *more refined* than that of a *Quotient of Ratios*, which latter you have seen that I identify with the conception of a *real Logarithm*, or a *logarithm of a positive number*. Accordingly, this train of speculation leads me to think that the notion of a *ratio of ratios* could not be *fully* developed without introducing *Logarithms of Negatives*, and therefore $\sqrt{-1}$, and so suggesting a *Double Algebra*. But I will not here pursue the subject, which may have already fatigued you.

'I may add, however, that while I think that a *Quaternion* is *best defined*, from a *geometrical* point of view, by saying that it is a *Quotient of Vectors*—although it may, with somewhat more of previous convention be regarded as a *product also*—I do not choose to call a quaternion a *Ratio of Vectors*. But *for ordinary purposes* ratios are so *connected* with quotients, that I have no objection to *say*, briefly, as is now usual, that the *ratio* of a to b is n, if it be that of n to 1.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *December* 31, 1863.

'I got yours of the 16th [17th], but have never read it all through till to-day. About *ratio* and *quotient*, I admit they are two different things—just as velocity—swiftness—and velocity, *v*, the *measure* of swiftness, are two different things. Ratio, with me, is a synonyme of *relative magnitude*, an idea too simple to be defined, or expressed without fearful abstraction. But as I. N. said of gravity—it *exists* and *acts*. Take a child, and say, "Now we are going to draw a house A. Now the chimney B." [A diagram with chimney out of proportion large.] The child says, "That chimney is *too big*." Too Big! why there is not a chimney in the town so small as this. Perhaps the child is posed; perhaps it says, "It is too big *for that house*." In either case the remark was dictated by the presence and action of the notion of relative magnitude.

'But when we come to measure relative magnitude we learn number, if we never learnt it before. Quotient is number.

'Euclid's λόγος is founded on the idea of communication. We talk about magnitude by *ratio*, which is a habitude of magnitude to magnitude κατα πηλικοτητα, *secundum quantuplicitatem*. That πηλικοτης meant QUANTUPLICITY and not *quantity*, is testified by Eutocius. That anything else makes nonsense of Euclid's definition, is testified by common sense.

'I believe Euclid, and still more Archimedes, had the germ of logarithmic language. But I incline to think that their ratio, when thus used, was an *instrument of alteration*; and that the duplicate ratio was the result of the ratio used twice. But the history of composition of ratio, as it appears in Euclid, is curious. Nothing can be more incomplete.

'[P. S.]—How does the milkman act like Pharaoh's daughter?
He gets a little profit from the water.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *April* 24, 1864.

'I have got hold of a theorem, just gone or going to Cambridge, which throws some light on $1-1+1-1+\ldots$.*

'... I am just sending a Paper on *Infinity* to Cambridge.† I shall shock all the mathematical prudentials by standing up for the bare uncloaked infinitesimals, great and small. I *know* they are subjectively mine; but I admit they may not be so to others.

'[*Apropos* of the Shakespeare tercentenary celebration.‡] For Miss Hamilton. What is the proper name for a young lady's first ball after she comes out? *Ans.* Her turn-se'enteenary festival.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *June* 8, 1864.

'I have never intentionally destroyed any letter or note of yours, but have in some way mislaid your last, which reached me (I think) in April.

'In the course of an extensive search for it, I have lit on a

* *Transactions of the Cambridge Philosophical Society*, vol. xi., p. 145.

† *Id.*, p. 190.

‡ I omitted in my biographical record of 1864 to mention that this celebration stirred up Hamilton's old, but never extinct, though dormant, interest in the great poet, to whom in an early sonnet he had made no unworthy reference (vol. i., p. 489). In his Journal for Saturday, the 23rd of April, I find this entry:—'Could not but remember a former 23rd of April (*supra*, pp. 97, 98), when there was what (in my own mind) I called a *Feast of the Poets*, Mrs. Wilde, now Lady Wilde, being one of them. Even then I associated (mentally) the day with Shakespeare and Wordsworth. [Wordsworth died on Shakespeare's birth- and death-day.] We had some *Shakespeare Readings* among ourselves, in the evening; I also read a good deal for myself, in and about Shakespeare.' He refers to his having subscribed to the celebration, and notes with interest Archbishop Trench's and Bishop Charles Wordsworth's sermons on the occasion, and the book by the latter on Shakespeare's knowledge of the Bible. On the 27th of April he writes:—'Another book of a very different kind [from Dobson on *Masonry*], which I have been lately reading, with much interest, many parts of, is the book of "*Select Specimens of the English Poets*, with Biographical Notices, &c. Edited by Aubrey De Vere, Esq." (London, 1858.) It was given to me in 1860 *by himself*; and I think that it does him great credit. I may take an opportunity of writing to him soon, perhaps about *Shakespeare*.'—R.P.G.

curious little pamphlet of 1857, the title of which I shall copy for you in full, if you have the slightest curiosity about it. It professes to be a Report, by a short-hand writer, of a "Drawing-room Discussion between a Protestant Clergyman and a Roman Catholic Priest, arising out of a challenge given by the latter," and it seems to be strictly fair; the blunders being distributed with the most absolute impartiality.

'What tickled me specially was the following little bit of Logic, which I am about to copy for you; premising that *I* write A and B, as the names of the two combatants.

'A. "The middle term can be taken once generally. If you wish to go to logic, I will meet you there."

'B. "The middle term may not be taken both times universally."

'After this little flourish of unaccustomed weapons, by common consent it seems that *Logic* was *dropped*. What strikes me as particularly rich is, that B has, in the pamphlet, the air of having *refuted*, or at least controverted, A. That B was wrong in asserting, and A in all probability wrong in thought—each having forgotten *Darapti*—is quite immaterial for the purpose of this note. What I invite you to do is, to tell me *which* was the Protestant, and which the Papist. If you had ever spent, as I once did, a night—the richest night of my life—with Father Tom, you could decide at once. Did you ever read in Blackwood (May, 1838) the Article entitled, "Father Tom and the Pope; or, a Night at the Vatican"? If so, you cannot have enjoyed it as I have done, who am an Irishman, and actually *won a wager* (never paid, of course) from the said Father Tom.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *June* 22, 1864.

'Your question is—Given an absurd logician, Protestant or Catholic, which?

'I incline to think he is more likely to be the Protestant. 1. The odds are greater that a Catholic has been educated on the older plan, and is less likely to bungle a technicality by sheer misunderstanding. 2. The Catholics can afford better logic than the Protestants. The authority of the Church is a manufactory of

premisses—so that they are not so much driven to logical shifts as are Protestants, who are obliged to give inferences from what are called *texts*, which are as often as not in defiance of all logic.

'I also have my theorem that the middle term cannot *twice* enter universally, that is, in a fundamental syllogism. When the thing happens the syllogism is always what I call *strengthened;* that is, one of the premises stronger than it need be to produce the conclusion. Such a syllogism may have both middles *particular*, or both *universal:* . . .

'I want a word to signify the value of x, which makes ϕx *infinite*. I shall adopt *antiroot* of ϕx, unless somebody proposes a better.

'Again, in $\rho(\cos\theta + \sin\theta . \sqrt{-1})$, ρ being the *modulus*, I want a word for the other factor. I shall use *signal factor* with reservation as aforesaid, and *signal angle* for θ. Shortened into *signal*, perhaps.

'I write from my examination room, with half my single eye on my class, and half on the paper.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *June* 24, 1864.

'I understand you to infer that the *obviously* illogical disputant was the Protestant. You were right, if (as I suppose) you meant my "B." If the question had been put to me, I should have inferred, or at least thought, which would have been right, that "A" was the Papist; agreeing, of course, in the result, with you. Only, as to the *step*, there would have been this difference: that from the sort of *swagger* with which the *first* mention of *logic* was made, as a sort of defiance to the other combatant, I should have *guessed* "A" to be a worthy, though incomparably inferior, colleague (or co-sacerdot) in Ireland, of *Father Tom;* who is, I assure you, notwithstanding any such faults of manner (if they were such), as I have alluded to, remembered by many to this day with great regard. But about the *obviousness* of "B"'s mistake: I entirely admit that "Darapti," which both "A" and "B" appear to have forgotten, is an *imperfect* (I mean an *imperfectly good*) syllogism, though not an *illegitimate* one. If *that* had not been seen and felt long ago, why should we have the *word* Darapti, with its obvious reference, by extensive reduction, to the *perfect* syllogism Darii?

And what can you mean by saying that the middle term *may* be *twice particular*, in a *legitimate* syllogism, as I suppose? . . .

'*June* 25 . . . I see no objection to your calling x the "*antiroot*," or *an* antiroot, of ϕx, when $\phi x = \infty$. In that case, however, we ought to call x a *root* of ϕx, when $\phi x = 0$; but even this is not yet usual.

'If in such an expression as $\rho(\cos\theta + \sqrt{-1}\sin\theta)$, in which ρ and θ are reals of the ordinary algebraical kind, and ρ, at least, is also *positive*, you retain *Argand's* (*not* Cauchy's) name of *modulus* ("module") for the factor ρ, and call the *other* a "*signal-factor*," you will be in strict conformity with Argand's recorded views. [Hamilton here quotes the two extracts from Argand made at the foot of p. (56) of his *Lectures on Quaternions*.] Argand had therefore anticipated the *thought* involved in your proposed *word* "signal," which seems to me (for *Double* Algebra) a very good one —as applied to the factor $\cos\theta + \sqrt{-1}\sin\theta$.

'You probably have Peacock's "Symbolical Algebra" (Cambridge, 1845) vol. ii. of a Treatise on Algebra. In page 204 you will find the passage:—"In the first place, in expressions such as $(\cos\theta \pm \sqrt{-1}\sin\theta)\rho$, we must consider $\cos\theta \pm \sqrt{-1}\sin\theta$ as a sign of affection of ρ, and as in no respect affecting its magnitude; for . . ." In p. 205, he speaks of $\cos\theta + \sqrt{-1}\sin\theta$ and $\cos\theta - \sqrt{-1}\sin\theta$, as "those signs." In short, Peacock (like Argand) considered *such signs* to be *extensions* of + 1 and − 1, or of + and −. I observe that in pages xxviii, xxix, of the Preface to Peacock's Algebra of 1830, he says, "We shall thus no longer be confined to + and − as the only signs of affection, inasmuch as we have created other signs." In fine, although the word *signal* (in your sense) is *new*, so far as I know, you can amply *justify* it, if you think it worth while, by an earlier usage as to the word *sign*.

'A quaternion

$$q = w + ix + jy + kz,$$

with my laws of i, j, k, *may* be reduced generally to the *form*,

$$q = r(\cos\theta + \sqrt{-1}\sin\theta);$$

in which

$$r\cos\theta = w,$$

$$r\sin\theta = \sqrt{(x^2 + y^2 + z^2)},$$

and

$$\sqrt{-1} = \frac{ix + jy + kz}{\sqrt{(x^2 + y^2 + z^2)}}.$$

This $\sqrt{-1}$ is just *as real* as that of Double Algebra; but it is intolerably *vague*, since it represents (on my principles) *any unit-line in space*, or *any directed radius* of an *unit-sphere*. Accordingly, I have long found it to be convenient, in practice, to *reserve* the *symbol* $\sqrt{-1}$ for its *old use* in algebra (compare p. iv of my new *Table of Contents*), as denoting a *non-real*, which has only a *symbolical existence*, and of which the essence is contained in the equation

$$(\sqrt{-1})^2 = -1;$$

except that $+\sqrt{-1}$ is supposed to be *unequal* and *opposite* to $-\sqrt{-1}$ in the (again purely *symbolical*) *sense* that

$$+\sqrt{-1} - \sqrt{-1} = 0.$$

'I have long thought it to be a *fault* of Double Algebra that it *interprets too much*. There *ought* to be *uninterpreted symbols;* and if you insist on treating $\pm\sqrt{-1}$ as representing *two real unit-lines*, at right angles to the two lines ± 1, in a given plane, you will just find yourself obliged to *invent* some *new signs* for the *old imaginaries* of *algebra*, in order to meet the necessities of *geometry*, even within the plane. [Hamilton then gives a case exemplifying the difficulty, and adds, "What would Double Algebra say to this?" After supplying what he supposes would be the answer resorted to, he concludes: "but you will not get Chasles or Salmon to adopt the geometrical consequences." The letter proceeds:] I remember remarking something of this sort to you, before the *Lectures* were published; and your replying to this effect: "Let us *close one account*, before we *open another*." The reply has great weight, no doubt; but the *new account must be opened*, whether by quaternions, or by some system or method hitherto undiscovered. I entirely admit that your "Trigonometry and Double Algebra" (1849) is not only an always *able*, and in *many* respects *original* work, but also that it must be a *useful* one to *students*, as clearly showing that there is *at least one sense* in which the *symbol* $\sqrt{-1}$ *can* be *fully interpreted*. Perhaps, if *algebra alone* were in question, it might be needless to go *farther*. But *algebraic geometry* has *other wants*, and peremptorily *requires imaginaries*.

'My *i*, *j*, *k*, are *not imaginaries*, though just *at the very first* (in 1843) I *called* them so, as being *each a value* of $\sqrt{-1}$ (as also are

$-i, -j, -k$, and generally $ix+jy+kz$, if $x^2+y^2+z^2=1$). They are quite *as real* as x, y, z themselves; and, in fact, I habitually interpret them as three rectangular unit-lines. [After further discussing the subject at considerable length, he ends with the following statement:—] When I first found (nearly twenty years ago) *imaginaries in quaternions* (distinct from i, j, k, which you know I have very long considered as *reals*), or rather in the *applications* of quaternions to *geometry*, my first impression was that I was likely thus to light on some *new set of geometrical imaginaries;* so little faith had I *then* in the *old set* of them, except as *creatures of old calculation.* But, to my great astonishment, *not one such instance* presented itself. I mentioned this to Salmon, at some dinner long ago; and *he* replied, that if the quaternions had *not borne this test*, he would have considered them as *good for nothing.*'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *June* 26, 1864.

'. . . Have you never known that some syllogisms have both middles particular when privative terms are allowed? This has been the case in everything I have written on logic.

'If a syllogism can have both middles universal, then, by using the privative of the middle, it is particular in both terms. . . .'

'Of course, no doubly particular middle occurs in an Aristotelian form . . .

'*July* 2 . . . The double algebra is an application of geometry to algebra by which full meaning is given to all symbols.

'I fully appreciate the imaginary intersections, asymptotes, &c.; but I attach no idea to them. There are algebraical forms which satisfy two equations, which, involving $\sqrt{-1}$, have no interpretation. There are, if you please, imaginary intersections of the curves; that is, no intersections at all. But perhaps a better word than *imaginary* might be found. But the instruction derived from the relations remains, be the name ever so disputable.

'. . . Double algebra is complete, as *one* of the perfect common algebras. Quaternions, I have told you all along, are *one* of the triple algebras.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 4, 1864.

'You were pleased, in a recent letter, to allude to your "single eye"; as you have also done sometimes in print. Now it would interest me very much to be allowed to compare the phenomena of *your* vision with those of my own. To be sure, I have *two eyes*, which both serve me still (without convex glasses) for the *very smallest print* I can find in any book; and which, on the other hand, are becoming, through age, by degrees more adapted than they used to be, to the enjoyment of a *prospect*, without the aid of concave glasses . . . But *being a two-eyed man*, I am also *habitually*, and from an earlier period of my life than I can distinctly remember, a *double-seer*, except for a page of print or manuscript before me. This I attribute mainly to my having been given, when I was a very young child, an excellent sliding Dollond, which packed into a rather thick case of perhaps only half a foot in length, but drew out into a telescope a yard long or thereabouts; and with which, when I was certainly not more than *ten*—but I *think* even earlier—I used to see Jupiter's moons *easily*, and Saturn's ring (though of course I could not *divide* it) without any doubt; besides observing, a year or two later, occultations of the Pleiades by *our* moon.

[Here follow particulars showing that Hamilton was, to use his own terms, 'not so much a *binoculist* as a *double monoculist*.' These are not here inserted, as they are substantially the same as are contained in his letter of June 14, 1864, to Dr. Ingleby, which will be found *supra*, pp. 175, 181.] . . .

'And therefore you may conceive how it happens that I *may* have—and in fact *have*—a real curiosity to compare notes with *you*, on the whole subject of *sight*.

'I. For instance, I entirely *reject* the very generally received hypothesis, that what we learn from this sense—or what we perceive, or seem to perceive by it—is *mainly*, or indeed (so far as I know) *at all*, with *me*, a result of *Binocular Vision*: such having been as stated, an *abnormal condition*, in my own case, from child-

hood. Will you indulge me with an answer to the question—Have you had only *one* useful eye, as long as you can *remember;* or has an accident, or an illness, deprived you of the sight of the other, within a time to which your memory extends? Do you trace anything, in your present visual perceptions or sensations, to a *recollection* of having had *two* seeing eyes?

'II. Are *you* conscious of *much* obligation to *touch*? I am *not;* and so audaciously has the claim (for touch) been made and pushed, by clever writers, that I am *almost* disposed to *reject it also altogether.*

'III. About *visible distance.*—I am not to be put off with "consent of philosophers"; or with Berkeley's argument that "distance is a line turned endways to the eye"; or in fact with *anything* which *assumes* the existence of an *eye* at all! I *do not see my eyes;* if I look at the *images* of them in a mirror, or at the eyes of *somebody else* in his head, it is merely a *part* of the general *picture* which I see. Such parts are also my hands or legs if I look down: I *never see myself. To* VISION—although not more so than to any other sense—I *am,* in strictness, *non-existent. Dogs see,* I suppose, as well as men do, and generally smell much better; but I do not believe that they ever form the *notion* of "EGO." It would therefore be absurd, on my own showing, for me to pretend to *see distance,* as *from myself,* who am to myself *invisible.* So far then I agree in the result with Berkeley. But yet I insist that every time I open my eyes, or *one* of them, in the light, *I do see distance;* in the sense that besides *colour* and *outline,* I see RELIEF, PERSPECTIVE; which I believe to be an *independent property of vision,* and *not acquired* through the help of *touch,* although *locomotion* has certainly assisted, by enabling me to go *round* objects, and *near* them. This assertion, that (*with each eye*) I SEE RELIEF, is quite consistent with the admission that I *do not see* a *tree,* or other object, as being *so many feet* or *yards away.* Such *estimates* I do not often make; and *when* I make them, I *know* that I am *judging,* well or ill, by *experience;* to the formation of which last, *some aid*—though in my own case, as I believe, *a very insignificant one*—has been derived from the sense of touch. Can I not *remember,* without having *handled* a measuring *rod,* that so many average *steps* took me from place A to object B, on *reaching* which I grant that I may naturally have put out my *hand;* and then make a good or

bad *guess* the next time, *how many steps* would take me from C to D?—this *guess* to be *corrected* by *trial*, and so on, if the process seem worth the continuing for the purpose of gaining skill. Does your self-observation, on the whole, agree with mine on the subject of *visible relief*?

'IV. Do you find yourself (*naturally* and *irresistibly*) believing that you *see* the *very trees* (or houses) *themselves*, at which you *look*? *Such* I observe to be my *natural impression*, even *now*, when I *cover either eye* with a hand; but it is otherwise when I look with *both*. A "Natural Realist" (*vide* Sir. W. H.) might ask me, "Will you not *believe your eyes*?"—to whom my answer would be: "Certainly NOT." *One* eye at a time I catch myself *involuntarily believing*, whatever *theories* I may have read or formed; but I CAN'T *believe both at once*, because they *contradict each other*. I *see*, at this moment, *two young haycocks* where I know that there is only *one*; and (in my present mood) it is no satisfaction to me to be informed, from *books*, that this is *because* there are *two retinal images* (NOT haycocks *themselves*) and that the *two optic axes do not converge*. Forgive this long letter, and believe me to remain,' &c.

[As a postscript Hamilton quotes from his letter to Dr. Ingleby, above referred to, the final paragraph beginning, 'To wind up the statement . . .' *Supra*, p. 181].

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'91, ADELAIDE ROAD, *July* 17, 1864.

'First as to my eye. When I was in preparation, my mother attended much to a favourite native servant (in India) who had the ophthalmia, which they call the *country sore eyes*. When I was born it was found I had had it too, and one eye was not destroyed, but never completely formed: it is only a rudiment, with a discoloration in the centre, which shows that nature intended a pupil. The eyelid is of a different size from the other [illustration]. This is slightly exaggerated. I have been offered a binocular glass at the opera, &c., scores of times, by people who had known me for years, with recommendation to try if it were not better than my own. So that the thing does not show much, for which no doubt spectacles are partly answerable.

'Accordingly I have always been strictly unocular. I have seen as much with my right eye as with any one finger—no more, and no less. I am very short-sighted, and more so, I think, as I get older. Without spectacles my reading distance would be less than six inches for moderate type, or my own handwriting. Four inches would be agreeable and convenient. My eye bears any amount of work without fatigue. But on principle I have avoided anything like frequent use of a telescope, which is an insidious foe.

'Now I am not aware of any use of sight in which I differ from a short-sighted person with two eyes.

'I have heard the theory—Berkeley's?—that perception of distance depends on the action of two eyes. I judge of distance just like another person. Also that *solidity* depends on two eyes. I never felt any want. If everything were *flat*, as I am told it is with me, I should not be disgusted with a *flat* picture. Now I have greater enjoyment in the *relief* of good perspective than in anything else connected with a picture.

'I am not aware of any particular obligation to touch. I am certainly not more indebted to it than others. It is very difficult to say what we owe to it.

'I am, as I said, perfectly unconscious of any want except a longer sight. I have heard it said that people with one eye cannot draw a straight line as well as with two. This* is dashed off. And my pupils used to remark that my chalk circles on the slate were very true, and joined at the ends to admiration.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 19, 1864.

'. . . Allow me to thank you *at once* for your most interesting account of your *eyes*—perfect and rudimentary. If you had not given me some preliminary encouragement, by your printed and unprinted writings, I should certainly not have presumed to *ask* you for any such account, which I shall be very glad indeed to have extended at your leisure. The subject appears to me to be one of high philosophical interest; and I still think that a comparison of your mode of vision with my own may be instructive, since *both*

* Illustrated by an admirably straight line.

differ so much from what is *stated* to be the condition of non-blind men in general.

'As to the mere *words*, I thankfully accept your gently suggested substitute, "unocular" for my hybrid term "monocular." I shall henceforth use the former; but in extenuation let me say that Chasles, who knows both Greek and Latin, has habitually indulged in the not less improper compound of "*homofocal*"; whereas he ought to have written, like Salmon and others, *con*focal. Since you are, for the moment, in the Chair of Criticism, should you object to the phrase "bin-unocular" to describe my own condition? It has only this moment occurred to me, and I may probably see objections to it to-morrow.

'Your letter has been *very* satisfactory to me, but I have still a few questions to ask. In a day or two I hope to start with my daughter—Lady H. not being *quite up* to it—on a little excursion to the county Wicklow.* But please address as usual when you can spare time to write.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *July* 31, 1864.

'As to your No. iv., I do, when I do not think about the matter, see the very objects; but I never find this notion stand the least consideration. To you, who see two things, it must be impossible to believe your eyes.

'In like manner, for common use, I put the representative *words* in place of the objects, and am content to say *man* is *animal*, which I know it *is not*. But thing signified by *man* is (some) thing signified by *animal*.

'I have no doubt, any more than you, that had I been only a *seeing post*, without touch, I should have recognised distance. I should have been cognisant of a *more* and a *less* of separation, of gradual diminution accompanying recession, and so on. But I do not know whether I should have gained the notion of distance as we *now have it*. It is very difficult to say what our notions of

* On July 25 Hamilton wrote to De Morgan from Glendalough a letter giving some account of this excursion. The portion of it which continues to possess interest will be found *supra*, p. 163.

the external would have been if we had never had a hand to put out.

'I cannot ascertain that I am, in any one point, different from you, or that a second eye would give me any other knowledge.

'I have no doubt that double sight is more intense in quality, as thus: I am beginning to bathe my feet, and I put one foot into the water, which is just as hot as I can bear it. When I put in the second foot, I *cannot* bear it. I have learnt nothing new about the pain but quantity. I suppose a second eye would add to the quantity of nervous action. I have never ascertained whether binoculists can see longer than myself without a candle when the sun goes down. I suppose by my never having had my attention turned to this that there is not much difference.

'My eye is so regular a thing that I think little about it. It is not once in five years that I have a day's weakness or irritation, and then only clearly in connexion with cold or feverish action on the system. But three weeks ago there was a novelty.

'My wife has taken with much advantage the peroxide (binoxide O^2H) of hydrogen. But she found that continuance weakened the eyes. But of this I knew nothing till afterwards. Feeling depressed by the heat, I took twenty drops—not an excessive dose—in water. The very next day I had, for the first time in my life, the *volitans*, I cannot say *musca*. My attendant began something like this [sketch of an irregular ring] and flitted about perpetually. It has been gradually diminishing, and is now like [sketch of a similar ring, smaller and fainter].

'This peroxide is a very valuable medicine—taken in food. I should add that it restored my dimensions in ten minutes. I had begun to lose the idea of occupying space, and felt as if I could live in a cocoon.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 4, 1864.

'. . . In your "Formal Logic" you allude, several times, to Hamilton's Notes on Reid; or rather you *cite* them. May I ask whether *your* copy, if dated 1846, *terminates abruptly* in the middle of a note, and of a sentence, at the end of page 914? If so, do you know whether any continuation has been since published?

Were it only from the circumstance of *your* controversial relations with Sir William Hamilton, I should like to know more of *his* writings generally. But in *philosophical*, as distinguished from purely *logical*, speculations, my own feeling of interest is of very old date indeed.'

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *July* 5, 1864.

'My edition of Reid by Hamilton ends in the middle of a sentence; I never heard of that sentence being completed, and do not believe in any such thing. . . .

'I perfectly agree with you that H. is a most interesting writer—even in logic, when quantity is not in question. On this point his head is a curious one. But as to other things the freshness of his mind, loaded as it was with learning, is unique. When at Oxford, he took up for his degree so many books which were quite out of the power of his examiners, that they left off and gave him a compliment instead of an examination.

'You would be surprised to know, by examination, what an original thought it was to put the sign of quantity to the predicate. The predecessors who have declared the predicate of an affirmative to be particular, and have said that "some" is the sign of particular quantity, have in no one instance written down "All men are *some* animals," even to reject it. It is clear they never thought of it—it never struck them nor they it.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *July* 9, 1864.

'. . . You know that I have recently re-borrowed Hamilton's "Reid"; and I must say that I agree, *at present*, much more with Ingleby than with Hamilton on the great question of our *Perception of an External Universe*.

'I am much more to be described, at present, in H.'s nomenclature—which I don't quite *like*, but do not find embarrassing—as a "*Cosmothetic Idealist*," like Ingleby, than a "Natural Realist," as H. at least *supposed himself to be*; although it is just now incomprehensible to me how Hamilton reconciled that *classification*

of his own views with his subtle *physiological theory*—adopted also into *his psychology*—of our being *in contact with the outward world*, through a highly refined ORGANISM; which (as described by him) is *wholly unknown* to *people in general;* and of which (as so described) the *belief* forms certainly *no part* of his favourite "COMMON SENSE."

'Differing from H. on a point so important as "Perception," you may guess that I differ from him (at present) on *many others* also; but I find his edition of "Reid" an eminently *useful* (as well as *entertaining*) book, because it is to me an eminently *clear* (as well as *learned*) one. A few technical terms not terrifying me, and the quotations being pleasant to read, I seem to myself to know, very distinctly, in almost every case, *what he meant;* and can generally say, after a little self-examination, *whether I agree with him or not.* I like *decided* statements, right or wrong: one knows what one is about when dealing with them. Sir W. H. assists me in *taking stock* of *my own present beliefs* on subjects of philosophy.

'Profound as his ignorance appears to have been respecting the *science* of *geometry*, it seems not impossible that his opinion respecting *editions* (including *manuscript ones*) of the *book*, "Euclid," may have a value from his admitted scholarship. And it runs in my head—though I cannot at this moment verify it by reference to a page—that H. has somewhere said in a note to "Reid," that the *Axioms* (and perhaps the *Definitions*) which we now find in copies of Euclid were the work of some "mendacious Editor." I own that I have long, from *internal evidence*, entertained a *suspicion* of their *non-genuineness*, however *honestly inserted* they may have been. The division into (*canonical*) *Books* and *Propositions* (for I waive the question of the *apocryphal* writings) may, probably, have been *Euclid's own.* But the *rest* seems to me likely enough to have been *added* since, though probably very long ago, by some *editor* or *commentator*, to make the work look more systematic. Such addition may have been of a piece with those *references* to preceding propositions, which do not seem to have been given by Euclid and Apollonius *themselves*, but are supplied in the *margins* of most printed editions of the former, and are incorporated with Halley's Latin *translation* of the latter (Oxford, 1710). *Apollonius*, however, *did* probably *define;* and Euclid *may* have done so. Indeed, I suppose on reflection that Euclid is *likely* to have given

definitions, but that they have not been handed down to me without *some corruptions*, and perhaps some *insertions*. The Axioms I could quite give up. On the whole, however, I attach no value to any opinion of *mine* in this matter.

'*September* 23.—I have not yet returned Hamilton's "Reid," though I shall probably do so this very day. I continue to admire Hamilton himself, as a writer, and should gladly be a *student* of his works, for many years to come; but am satisfied that I should never become a *pupil*. His whole *view* of what PHILOSOPHY is, or ought to be, differs essentially from mine. But it would not become me to enlarge at present on such a topic.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'91, ADELAIDE ROAD, *September* 22, 1864.

'. . . I have nothing to say about myself or anybody else. At the back of my letter [in the *Athenæum*] of September 17, abovementioned, you will see Herschel's stinging answer to a request to him to sign a declaration. I do not know whether you were asked —I was not. I suspect the result will be a warning not to apply to science to make declarations which are—under very distorted phrases—intended to support ecclesiastics in [exhi]biting their sinuosity.'

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *September* 23, 1864.

'. . . I hope that people will not send me the circular, which Sir J. Herschel has so properly refused to sign. Of course, I too should refuse; but I might think it necessary to send my letter of refusal to be printed, and that would be a bore.

'P.S.—It is a *curiosity*. I think of *copying* it into some blank book of my own. Does anyone know who drew it up? At least we are not in *this* case asked to sign for "the love of God."'

From the SAME *to the* SAME.

'OBSERVATORY, *September* 30, 1864.

'In your note of the 22nd I observe that you speak of "Herschel's stinging answer"; and the *Athenæum* of September 17 reports Sir J. F. W. Herschel as having written: "But I consider this movement simply mischievous," &c. All this looks as if *you* thought—and as if *he* too *thought*—that he not merely declined to *sign*, but even, to *some* extent, disapproved the declaration. I find, however, from a Dublin paper which reached me yesterday, that it has been recently discovered in London, and the discovery published in the *Press* newspaper (date not given) as part of an article on "Science and Revelation," that Sir John Herschel (whatever he may have *supposed* himself to mean) *meant no such thing*.

'The following is a sufficient extract:—"The two chief dissentients from the well-intentioned address are Sir John Herschel and Sir John Bowring. Their replies exhibit some slight inconsistency—for, while they refuse the authority to be derived from the apposition of their signatures, they express their complete concurrence in the principles and sentiments of the declaration. Sir John Herschel declines for the sake of others rather than himself. He regards the address in the light of a semi-literary semi-religious test, and resents its introduction, not as a burden to himself, but as grating on the feelings of other estimable men with all the harshness of controversial hostility. Sir John Bowring, while professing adhesion to the terms of the document, replies in a less amiable and less conciliatory spirit." In fact, I do not see how the body of Sir John Bowring's answer is reconciled with his first sentence. But it is certainly *news* (from London) that Sir John Herschel expressed, in his letter, his *complete concurrence* in the *principles* and *sentiments* of the declaration. Hodiernal hermeneutics are advancing.

'I am happy to say, that I have not been hitherto asked to *sign* the document; perhaps an *Astronomer* is regarded as scarcely a *Student* of the *Natural Sciences*. If I were Professor of *Mathematics*, I might have a still better chance of escaping. But perhaps there will come, after some time, a *printed circular* to each of us, with many names attached. *Nous verrons.*'

[Sir W. R. Hamilton was asked to sign the declaration. His reply will be found *supra*, p. 191. The above letter is the last written by Hamilton in this Correspondence which has been preserved; it will be seen, however, by the letters which follow from De Morgan that the latter had received some of subsequent date.]

From A. De Morgan *to* Sir W. R. Hamilton.

'91, Adelaide Road, *October* 2, 1864.

'I take Herschel to object to the declaration as mischievous—independently of the truth or falsehood of the matter. Bowring, agreeing with Herschel in the recondite discovery that two truths cannot contradict each other, puts forward more distinctly his belief that the truth of revelation has been dormant in "contented ignorance."

'The declaration is so illogically worded that no one who analyses it will sign it. But it contains some propositions which every believer in revelation admits—nay, which unbelievers admit conditionally. But H. objects to it because it means this:—

'Declaration translated into English:—

'"Colenso and others are inquiring into the distinction between historical record and revealed doctrine. This won't do; our system is so shaky that it will not stand substantial repair. Why! we dare not knock in a nail, for fear the rotten beam should crumble. We must have a declaration that two truths cannot disagree. Some must sign for love, and more for fear; we must say in conversation that none but an atheist would refuse to sign."

'H. and B. know very well that there is something like this under all.

'There is an old dispute which I find in an old writing of the first century which tallies exactly. Colenso and the rest go about saying: ὅτι οὐκ εἰσὶ θεοὶ οἱ διὰ χειρῶν γενόμενοι. That is, that records or traditions, which might be written by men, are not revelations from God. The craftsmen who get εὐπορία—which sums into 666—from the system, meet together in the theatre, and cry for two hours: Μεγάλη ἡ Ἄρτεμις Ἐφεσίων. There is no sensible γραμματεὺς to call to order, so H. and B. have taken the office, in which, indeed, Daubeny had preceded.

'Herschel refers to his own opinions on this subject, printed more than thirty years ago. He is all right! except that he uses the common mistranslation of *cui bono?* I set him right the other day, and he tells me he never heard the real translation.

'The old school of theology must have been very hard up for something to do when they contrived this declaration. If their clergy-declaration—with every influence of love and fear—only produced half the clergy, what could they have expected to do among educated laity? Their list of names—that put forward as the choice lot—is a very queer sample. . . . You are not let off as an Astronomer: Main is there; and you would have been asked, if they thought there had been a chance. And they would have had professors of mathematics. But, between you and me, mathematicians and astronomers are not looked upon as the *élite* of orthodoxy; physical science and natural history are judged more accessible.

'They asked Herschel, no doubt, because, over and above his really religious character, they thought him a timid man. They were much mistaken. H. is of a nervous and diffident temperament; but perfectly decided in his course of action.'

From the SAME *to the* SAME.

'91, ADELAIDE ROAD, *December* 13, 1864.

'There will be no need to tell you that you must be aiding and assisting in getting a pension for Boole's wife and daughters . . . An application will be made and must be well backed . . . As soon as I get materials from Cork, I intend to draw up something which I will send you for revision. . . .

'*December* 30, 1864.—Having letters to write on this matter, I can only just beg your immediate attention to the following:—

'It is decided to present a memorial from Cork College. This is to be backed by *testimonies* from men of science in separate letters—testimonies to Boole's eminence and success. The letters to be addressed and forwarded to Dr. Ryall, Vice-President of Queen's College, Cork. Pray write at once. I think there is fair hope of success; the more as the matter must go through the Irish Government, and Irish applicants sing over it in hundreds of cases in which English do whistle for it.

'In the railroad days an eminent parliamentary solicitor told me that on an Irish Railway Bill there were some half-dozen of standing orders not complied with. He pointed this out to the Irish solicitor who had come over, simply stating that the thing was lost for the session. "Is that all?" said the Irishman, "we'll move the House;" and no more would he say. And no more needed he to say, for an Irish Member moved the House, and it suspended all the standing orders for the asking. Whereat the English solicitor opened his eyes so wide that he has never been able to close them since. The House would have simply laughed at such an application on behalf of anything on this side of the canal. So I feel hopeful about Boole's case.

'*February* 3, 1865.—Before I did the like [writing a cheque] I wrote to Dr. Ryall, misdoubting that the subscription plan had the sanction of the relatives. A letter from him crossed mine, repudiating the scheme altogether, and you may see by a letter in the *Times* of to-day that the thing is to be dropped. Accordingly, I have crossed your cheque to Nogo and Co., and return it. Such a thing may be wanted yet, if the application to Government be unsuccessful; but Mrs. Boole is not yet in want, and has good hope of the application to Lord Palmerston. In the meantime they will all feel your kindness very warmly.'

CLOSE OF THE CORRESPONDENCE.

[Through an inadvertence, which I much regret, the following letters were not inserted in the place in the series which belongs to them. They ought to have come in immediately after the letter of April 16, 1852, from Professor De Morgan, printed on pages 252 and 253. I may also state here that I have lately come upon a detached postscript to De Morgan's letter of May 21, 1852, pages 355, 356. It contains his proof of

$$\frac{\pi}{\sin \pi n} = \int_0^\infty \frac{dx}{1+x^n},$$

and is referred to by Hamilton in his letter of May 24. De Morgan says of his proof, 'I swear by the above. Let me see who has a word to say against it. But being true, I suspect it cannot be new. I see a parcel of cousins of this process.']

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 19, 1852.

'After working, chiefly at my book, for at least twelve hours to-day—but during most of the time in the open air—I was wavering whether to throw myself, which I seldom do, upon a sofa, or in some other way take rest; when three things, in the way of variety, caught my eye. *Imprimis*, your double note, received this morning, and read with interest at the time; *secundo*, Herschel's very elegant "*Collection of Examples on the application of the Calculus of Finite Differences* (Cambridge, 1820)"; and *tertio*, a copy of *Guy Mannering*. I have dipped within these few minutes into all three, and am in a mood to write to you about them. Expect no regularity; conceive me to have dipped in the dark into an urn, with those subjects, *and others*, inscribed on pieces of paper. Yet let me, as suiting that very conception, *number* my sentences, or paragraphs, just as if they were, what happily they are not, the portions of a book, or essay.

'1. Please to interpret for me, if you can, or refer me to some place where you have interpreted the following passage in Sir

Walter Scott's Introduction to *Guy Mannering*:—"Were everything to happen in the ordinary train of events, the future would be subject to the rules of arithmetic, like the chances of gaming. But extraordinary events, and wonderful runs of luck, defy the calculations of mankind, and throw impenetrable darkness on future contingencies."

'2. I cannot find, in Herschel's Chapter on Continued Fractions, any trace of the following Theorem, which I, not long ago, perceived, as a result of the quaternion calculus, and which as such admits of curious interpretations, but which is valid also for arithmetic. Is it new?

'"If $u_x = \left(\frac{b}{a+b}\right)^x c$, and $v_x = \frac{u_x + q_2}{u_x + q_1}$ (where q_1 q_2 are [two] roots of the quadratic equation $q^2 = qa + b$), then $v_x = q_2 v_0 q_1^{-x}$."

'3. You are welcome to stare at the "two" roots of a quadratic; but you must know that I am not quite contented at there being *only six roots* (so far as I have yet been able to assign them —and so far as *that* I went in 1844, though I have only lately applied the result—) for a quadratic equation in quaternions of this particular form,

$$q^2 = qa + b.$$

I have recently ventured to print the assertion of my belief, that there are *ten missing roots*, which *probably are infinite*. For *in general* an *equation of the* n^{th} *degree*, in *quaternions*, ought to have n^4 roots, *real or imaginary*. In *sets* of the Sth *order*, such an equation should have n^s roots. Of the *six finite roots* of $q^2 = qa + b$, where *abq* are 3 quaternions, I found so long ago as the year lately mentioned, but am only now coming to print the investigation, that *two* are in general *real* quaternions, and that the *other four* are *imaginary*.

'*April* 20, 1852.—After writing what is on the foregoing sheet, I resumed *Gug Mannering*, and became fairly caught by it, though I must have read the book at least ten times before. This morning I am fresh for work again, but shall continue my letter to you, and shall, really for the sake of *freedom*, go on to number the paragraphs.

'4. Have you any faith in astrology? Perhaps this is not quite a fair question—if so, you can pass it by. I never tried to acquire the slightest knowledge of it; but a Moravian Minister, an uncle of mine by marriage, the Rev. John Willey, was supposed to be a believer, and to know more about astrology than astronomy, although he had wonderful patience and neatness in the graphical operations of the latter; for instance, so long ago as 1820, he sent to me, as to a promising lad, or one so considered, a beautiful drawing on a large scale of the solar eclipse which was to take place in the September (if I remember) of that year. But he was often obliged to apply to me afterwards for *rules*, easily investigated, for constructing other charts and views. I never asked him, in return, to cast my horoscope; but could have told him, as it happens, the exact hour (I might say minute) of my birth.'

[Here follows a half-sheet filled with particulars connected with his birth. As they have all been given in the biography, and as Hamilton desires De Morgan to burn this half-sheet I decide to omit it.

Paragraphs 5 to 24 occupy from page 5 to page 24, inclusive, of this letter.

I am indebted to Mr. Cathcart, F.T.C.D., for the following condensed statement of what is contained in these paragraphs:

'Hamilton asks whether "it ever occurred to De Morgan that if an *ordinary* algebraic equation of the nth degree in x be split into two equations by substituting $x'+iy'$ for x, and if y' be then eliminated, the result must generally be an equation of the degree n^2 in x'. Now, because x has n values, x' must have n real values, or this equation of degree n^2 must have n real roots. But what becomes of the other n^2-n roots?" He discusses this in detail for a quadratic and cubic, objecting to De Morgan's interpretation in Double Algebra as inconsistent with modern views in geometry, and insisting on the retention of an uninterpreted symbol $\sqrt{-1}$, in order that geometrically non-existent points may be treated as non-existent, although symbolised. This leads him to adumbrate a new species of quadruple algebra, or algebra of bi-couples, claiming in conclusion that this letter may be preserved, as it "may yet become a document in connexion with speculations on bi-couples."']

From the SAME *to the* SAME.

'OBSERVATORY, *April* 22, 1852.

'The correction about Fermat is important; and the anticipation by the old Bishop is very curious indeed.*

'While your mind is on those subjects—though I suppose the principles, and even the history of the Differential Calculus, are almost as familiar to you at one time as at another—I wish you would spend a minute on considering my definition of the differential of a function of a quaternion, which I gave to Charles Graves, a few years ago, nearly in the following terms, and made the basis of various investigations. Perhaps you may have met with it, almost word for word, in some book already published. I have made no search on the subject.

'The definition which I employ is this:

$$dfq = \lim_{n=\infty} . n \left\{ f\left(q + \frac{1}{n} dq\right) - fq \right\}.$$

Of course this is *true* in algebra, but I want to know whether it has been expressly proposed—perhaps I may have read and forgotten it. Its grand recommendation to *me* is, that it in no way introduces the commutative principle of multiplication, and is consequently free from all notion of differential *coefficients*. You see that it does not suppose dq nor dfq to be *small*. dfq is given by it under the form of a function of the *two* arbitrary quaternions, q and dq, which, however, is *linear* (or distributive) relatively to the latter, but is *not* generally reducible to the form of the *product* such as $f'q \,.\, dq$. This is important. For ex., . . .

'The postman knocks. Mathematicians little guess what an avalanche of problems is impending over their devoted heads—I have this morning integrated the equation in quaternions which represents an arbitrary surface of revolution.

'What you say about Grant is very interesting.

'*April* 24 . . . It tantalises me to think that, my book being already so big, I must reserve all my curiosities about differentials, variations, and integrals of quaternions, for some future occasion. . .'

* *Supra*, pp. 351, 353.

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 23, 1852.

'I will read the long letter soon. I answer the short one.

'Your mode of exhibiting the second term of the development of $f(q+dq)$—for that is what it is—is new to me. I have often seen the infinitely great explicitly substituted for the infinitely small in an integral, as in

$$\int_b^a fxdx = \frac{a-b}{n}\left\{fb + f\left(b+\frac{a-b}{n}\right)\right.$$
$$+f\left(b+2\frac{a-b}{n}\right)+\ldots \qquad (n=\infty$$
$$\left.+f\left(b+n\frac{a-b}{n}\right)\right\},$$

but never, to my recollection, have I seen it in differentiation.

'But why is this definition more corrective of the use of inversion in multiplication than the common one? If I choose, in $d\,.\,q^2$, I can proceed by $(q+dq)(q+dq)-q^2$, or $q^2+qdq+dq\,.\,q+dq^2-q^2$; or, rejecting dq^2,

$$qdq + dq\,.\,q.$$

So PQ gives $PdQ + dP\,.\,Q$, while QP gives $QdP + dQ\,.\,P$, and then I can invert or not as I choose to make my hypotheses.

'You get on too quick for poor mortals to follow. Your book must be out and I must have a year to think of it—at least—before I shall fully feel up to your manœuvres.

'Your namesake (-* Rowan) has fired another shot at me—to my great joy—in a printed book which will last. I always regretted that his mode of arguing against my views was in an ephemeral pamphlet and a letter in the *Athenæum*, and now he identifies it with himself ἐς ἀει in a big volume of philosophy. There will some day be a biographical comparison of the two Sir W. H.'s, for your respective ghosts to meet and talk over.

* Minus sign of exception. [In a note of De Morgan's, written September 25, 1862, he says, 'I get on better with you in matters of quantification than with the other of the name. It all lies in *Rowan*: the rowan-tree was a preservative against witchcraft, evil eye, &c. (*vid.* W. Scott).']

'I have never heard any facts in favour of astrology. I do not see its prophecies come true. I have investigated the methods, and find different authors wholly disagreeing. I cannot imagine how the astrologers manage to get different destinies for persons born at the same place and hour—twins for instance. If the few minutes which elapse between the birth of twins are enough to create wholly different destinies, it follows that their ordinary rules, in which a few minutes make no difference, must be futile. I would not decide *à priori* on astrology or anything else. If anybody would startle me with a good *definite* prophecy (fulfilled) I would look out for more evidence. But the modern astrologers are mere hinters of hint compared with the old ones.

'Walter Scott has blundered in *Guy Mannering*. He makes the astrologer merely ask for a *view* of the heavenly bodies, and from this view with the naked eye, without tables, he makes him calculate the destiny of young Bertram so closely, that the *very hour and minute* of his disaster agrees with that which he had previously prophesied for his own future wife—and the prediction comes true afterwards. Calculation to a minute from the optical appearance of the heavens is a feat which would beat the three Astronomers Royal. When they can determine the parallax of the moon by standing at the three vertices of an equilateral triangle and looking at the object, then they will be fit to compete with Guy Mannering.

'After what I have seen of clairvoyance, I will not reject the Egyptian stories. A cousin of mine asked one of these black sorcerers at Alexandria to describe *me* by name. He gave nothing but the name. The man immediately described a person sitting at a table reading by himself.

'There is no story so wonderful, but I am prepared to think it *possible*. This is a great step. I once knew all about what was possible and what was impossible. But I find it difficult to believe further than I see.'

From Sir W. R. Hamilton *to* A. De Morgan.

'Observatory, *April* 26, 1852.

'Your method of differentiating a function, without commutation of factors, by simply developing, and taking terms of first

dimension, would be the best, I grant (*vide* art. 572 *supra*,* which was in type before your letter arrived), if it could always be applied with even moderate ease. But you would oblige me if you would send me, even a month hence—you would still have anticipated me—any neat development of even the *square-root* of a sum $(r+q)^{\frac{1}{2}}$, where rq is not $= qr$. Yet I have long since found the *differential of the square-root of a quaternion*, and *applied* it, geometrically, some years ago.

'I lay no stress on the infinitely *great* value of n. It would suit me almost as well to define $dfq = \lim_{x=0} . x^{-1} \{f(q+xdq) - fx\}$, though I think the other form a little clearer. But the important thing is that I avoid—1st, commutation of factors; 2nd, development in series; 3rd, smallness of differentials. Perhaps you can give me (if so, I shall be glad to get it) another definition eluding these three points.'

From A. De Morgan *to* Sir W. R. Hamilton.

'7, Camden-street, Camden Town,
'*April* 27, 1852.

'. . . I will think about $(a+b)^{\frac{1}{2}}$, &c. But, like a true sceptic I first ask about its existence, under the given conditions.

'When n is integer we have, in $(a+b)^n$, not

$$\Sigma\, m_n\, a^m b^{n-m},$$

but every distribution of real permutation, as in

$$\begin{aligned}(a+b)^3 = {} & aaa + aab + aba + baa \\ & + abb + bab + bba \\ & + bbb.\end{aligned}$$

If then you can give us $(a+b)^n$ when n is fractional, you will, perhaps, tell us how to form all the combinations of m out of n, when m and n are both fractional, or n at least. At present I do not realise any possible form of development.'

* [This seems to be a mistake for *Lectures on Quaternions*; in the Elements there are no articles beyond 423; and art. 572 in the *Lectures* is apposite.]

From SIR W. R. HAMILTON *to* A. DE MORGAN.

'OBSERVATORY, *April* 29, 1852.

'I am charmed to find you sceptical about $(a+b)^{\frac{1}{2}}$; it was the point I wished to bring you to. I have no present faith in the possibility of a development, analogous to the usual one, for such a square-root, when a and b are arbitrary functional characteristics. Suppose $(\phi+\psi)^{\frac{1}{2}}$, or more particularly $(\log+\sin)^{\frac{1}{2}}$. Yet I do not despair of somebody discovering $\log^{\frac{1}{2}}$ or $\sin^{\frac{1}{2}}$. But for quaternions there *is* a development, though I scarcely know the third term.'

From A. DE MORGAN *to* SIR W. R. HAMILTON.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*April* 30, 1852.

'I have matured doubts about $\phi^{\frac{m}{n}}$, ϕ being a functional symbol. In my *Diff. Calc.*, p. 598, I have given what satisfies me we cannot now settle—

$$\frac{d^{\frac{m}{n}}y}{(dx)^{\frac{m}{n}}} \text{ (say } D^{\frac{m}{n}}y),$$

but must halt between an infinite number of cases. Observing how the *arbitrary introduction* (const. in integration, $\phi(\cos 2\pi x)$ in equation of differences) increases in indeterminateness at every step we make in the road; is it not natural to think this increase of indeterminateness of form must continue?

'The poorest and meanest functional equation has a terrible wide range of solution.

'If $\phi(x+1)=\phi x$ gives $\phi x=\chi(\cos 2\pi x)$—where χ is any function not inversive of cos—what may we look for in

$$\begin{cases} \phi(n+1, x)=\log \phi(n, \overleftrightarrow{x}) \\ \phi(o, x)=x, \end{cases}$$

and how are we to decide which of all the solutions is properly $\log^{\frac{m}{n}} x$?

'Look at the enormous extent of solutions of $\phi^2 x=x$, where $\phi x=\psi^{\frac{1}{2}}x$; ψx being x, and we are bewildered.

'We know that

$$\phi x = \chi(-\chi^{-1}x),$$

where χ is any function whatever. But we may hardly say this contains all—though the considerations in my Calculus of Functions seem to lead to a such conclusion.

'I do not doubt that there is a development of $\phi^{\frac{1}{2}}$ or $\phi^{\frac{m}{n}}$. My puzzle is to know which it is of an infinite number.

'Your case $(a + b)^{\frac{1}{2}}$, without $ab = ba$, is of the same school as $(\log + \sin)^{\frac{1}{2}}$, but let us be monomial before we are binomial, unless cause be shown. I doubt if you will do $(\log + \sin)^{\frac{m}{n}}$ before $(\log + a \sin)^{\frac{m}{n}}$, and this last includes $\log^{\frac{m}{n}}$.

'Where is Walker's Logic? The other Sir W. H. has recalled my attention to the subject.

From the SAME *to the* SAME.

'7, CAMDEN-STREET, CAMDEN TOWN,
'*May* 1, 1852.

'Your letter of April 19, five sheets long, you gave permission to me to defer, and I have availed myself of it liberally. But I happen to be disengaged for the moment, and it comes upon me as a pleasant occupation, to take it in your own division.

'I cannot interpret W. Scott's *Guy Mannering*:—"Were everything to happen in the ordinary train of events, the future would be subject to the rules of arithmetic, like the chances of gaming." I suspect that by ordinary train he means *equably*, head, tail, head, tail, &c. But the chances of gaming are not so calculable: in fact he is confused. The runs in gaming *precisely* resemble ordinary events. All runs. The other day I looked into some old volumes of the R.I.A., merely because they, and I, and an unoccupied moment came together; and for the first time in my life I saw the word *pemphigus*. That same evening I happened to light on a medical review—and saw the word again—for the second time.

'Some years ago I used to write in the *Dublin Review*, before the political events which have made it impossible to pull with them. The Editor and the now Cardinal were looking over papers, books, &c., and came upon a work on *book-keeping*, with a request for a notice. They both laughed heartily at the idea of

whom they could possibly apply to for such a job, and when their laugh was finished, took up the next packet. On opening this it turned out to be a MS. from me of an article on *book-keeping*, sent without any previous notice, and without any particular reason.

'The quaternions I must defer till the vacation, when, if I possibly can, I mean to sit at your feet till I feel strong enough to get up and walk.

'I think I answered on astrology and your birthday. I got a letter yesterday from a relation requesting I would forward to his daughter the places of the planets for certain days, which "a scientific friend" of hers wanted. Now on looking at the dates I saw one of them must be his own birthday, and I felt sure that he was going to astrologise, and that the "scientific friend" was dust for my eyes (or eye rather). So looking at the *Naut. Alm.* I found to my great satisfaction that the places are given for every *six* days, and I accordingly sent the adjacent days to the given day, saying that the scientific friend could no doubt *interpolate*, which will be Hebrew.

'Something like your case of $n^2 - n$ intruding roots does arise in the common way of producing Cardan's result. And nine roots are produced, of which only three are allowable, &c.'

[Unfortunately the concluding sheets of this letter have not been preserved.]

APPENDIX.

VOL. II., PAGE 537.

POPULAR ACCOUNT OF THE QUATERNION CALCULUS.

As it seems important to give in the words of its author what was intended by him to be a popular account of the principles on which the Quaternion Calculus was founded, I print here two documents of this nature: the unfinished letter to his uncle, the Rev. James Hamilton, of which mention has been made, *supra*, vol. ii. p. 537, and an *Elementary Sketch* written after the publication of his *Lectures*, and therefore to be accepted as a fully-matured exposition of its subject.

From SIR W. R. HAMILTON *to* REV. JAMES HAMILTON.

[FROM A COPY BY W. R. H.]

'OBSERVATORY, *September* 11, 1846.

'You have expressed a wish to see some written account of the principles of my theory of Quaternions, respecting which we have had conversations together on several different occasions. I shall endeavour to comply with your desire; but must first recall to your recollection some elements of that view of Algebra, which I published in the *Transactions* of the R. I. A. more than eleven years ago; and concerning which I had often talked and written to you, at periods preceding the date of that publication.

'According to that old view of mine, which I have seen no reason to abandon, the subject-matter of algebraic science is the abstract notion of Time; divested of, or not yet clothed with, any actual knowledge which we may possess of the real Events of History, or any conception which we may frame of Cause and Effect in Nature; but involving, what indeed it *cannot* be divested of, the thought of *possible* Succession, or of pure *ideal* Progression. It must be left to better metaphysicians and mathe-

maticians than myself to determine, whether I am right in regarding such a thought as properly expressed by the words "Pure Time," and as affording a sufficient basis for the erection of the edifice of Algebra. Meanwhile I adhere to my long-since published opinion, that Algebra is the Science of Pure Time; and in encouragement to those who may be disposed to give, on trial, some degree of attention to my views, may mention that it is admitted by other persons, of acknowledged competence to form some judgment on the subject, that the notion of *time* supplies at least a wide range of *illustration* to algebra, and that a large *part* of this science can be developed from this conception.

'It may also be accounted a presumption in favour of a system which introduces *from the outset*, without reserve or fear, the thought of time into the study of Algebra, that it prepares for welcoming as friends, instead of suspecting as intruders, those trains of speculation by which—this thought of time being the guide—Napier discovered Logarithms, and Newton Fluxions. In the speech respecting Quaternions which I made, last winter, to the Academy, the occasion warmed me into saying it was time to have done with the pedantry which censured Newton for the introduction of a foreign element. That element—the notion of *time*, according to which the discoverer was led to conceive fluxions as the *velocities* with which magnitudes vary—had been employed by the author of the *Canon Mirificus*, as well as by the author of the *Principia*. In my view, as in theirs—if it be not a presumption in me even to defend them—the thought to which those great men were indebted for their great discoveries in Algebra is one not alien, but native to the soil. And instead of seeking to attain consistency and uniformity of system, as some modern writers have attempted, by banishing this thought of time from the *higher* Algebra, I seek to attain the same object, by systematically introducing it into the *lower* or earlier parts of the science. Indeed I am profoundly impressed with the conviction that if the intuition or conception of time be put out of view, as foreign to Algebra, then Algebra itself must cease to be regarded as a *science*. It must either descend into the rank of an *art*, which has for its province to supply convenient rules of calculation; or pass, by what to some minds may seem an ascent, into the category of a language, having no independent *truth*, but only at most a certain coherency or elegance. The *symbols* will then become, what many now account them to be, the all-in-all of algebra: the analogy to geometry will disappear: the signs will have no longer a reference to things or thoughts signified by them.

'*September* 22, 1846.—I have not access, at this moment, to either the *Tractatus de Quadraturâ Curvarum*, by Sir Isaac Newton, which was published in London in 1704, nor the *Mirifici Logarithmorum Canonis Descriptio*, by Baron Napier, published at Edinburgh in 1614; but the

following extracts from those two memorable publications,* which I made in the College Library several years ago, may serve to justify, in an historical point of view, some of the assertions made in the preceding sheet of this letter.'

ELEMENTARY SKETCH OF THE NATURE OF THAT CONCEPTION OF MATHEMATICAL QUATERNIONS, WHICH IS DEVELOPED MORE IN DETAIL BY SIR W. R. HAMILTON, IN HIS RECENTLY PUBLISHED VOLUME OF LECTURES ON THAT SUBJECT.

'(1). The *word* "Quaternion" requires no explanation, since, although not now very commonly used, it occurs in the Scriptures and in Milton. Peter was delivered to "four quaternions of soldiers" to keep him; Adam, in his morning hymn, invokes air and the elements, "which in quaternion run." The word (like the Latin "quaternio," from which it is derived) means simply a *set of four*, whether those "four" be persons or things.

'(2). But the question arises, what special connexion has the *number Four* with mathematics generally, or with that branch of mathematical science in particular, to which the "Lectures on Quaternions" relate?

'(3). One general form of answer to this question is the following:—that in the mathematical quaternion is involved a peculiar synthesis, or combination, of the conceptions of *space* and *time;* and that while TIME is usually pictured or represented by metaphysicians under the figure of a *line*—a single stream with its ONE current—an unique axis of progression, SPACE is, on the contrary, imagined or conceived in connexion with THREE distinct axes, three lines at right angles to each other; such as the three edges of a room, which meet at a corner of the ceiling, or of the floor, one vertical and two horizontal; height, length, and breadth. In *time*, we have only the forward and the backward, looking before and after. In *space*, there is not merely the contrast between the directions of upward and downward, but also between those of southward and northward, and again between westward and eastward. Time is said to have only *one dimension*, and space to have *three dimensions*. The former is an *unidimensional*, the latter a *tridimensional progression*. The mathematical *quaternion* partakes of *both* these elements; in technical language it may be said to be "time plus space," or "space plus time": and in this sense it has, or at least it involves a reference to, *four dimensions*. In an

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* See for these extracts a note to the *General Introductory Remarks* prefixed by Sir W. R. Hamilton to his treatise on Algebra as the Science of Pure Time, in the *Transactions* of the Royal Irish Academy, vol. xvii., 1837.

unpublished sonnet to Sir John Herschel, entitled "The Tetractys" (a Greek word equivalent to the Latin Quaternio), the author of the Lectures introduced the two following lines, which give, in the shortest possible form, an expression of the view which has been in the foregoing remarks unfolded more at length:—

> "And how the One of Time, of Space the Three,
> Might in the Chain of Symbol girdled be."

'(4). Those who are entirely unacquainted with mathematical science may yet derive, from what has been above remarked, a sufficient preliminary insight into the nature of the speculations and inquiries to which the "Lectures on Quaternions" relate. A philosophical, if not a technically scientific, knowledge of the author's general aim, and of the idea which has guided him, may in this way be easily attained. But a very moderate acquaintance with the conceptions of geometry will suffice to render intelligible, from another point of view, the importance which the author attaches to the number Four in mathematics.

'(5). As early as the first book of Euclid's *Elements*, an attentive student is (or may be) led to consider the *relative length*, and also the *relative direction*, of one straight line as compared with another. Thus when Euclid shows, in his very first proposition, how to construct on a given base AB an equilateral triangle ABC, he virtually teaches how, when one line AB is proposed or *given*, to draw a *new line* BC (or AC), which shall in *length* be *equal* to the given one, and in *direction* shall make with it an *angle of sixty degrees*, namely, the angle ABC (or BAC), which is the third part of 180 degrees, or of two right angles.

'(6). In this elementary example, if the length of the given *base* AB be taken as the standard of length, and be on that account called *unity*, or *one*, then the length of the *side* BC (or AC) of the triangle must also be denoted by the same number, ONE; and these TWO NUMBERS, *one*, and *sixty*, serve in this view to define, or to describe, the *length and direction* of the new or constructed line BC; at least if the latter number (sixty) be combined with the consideration of a certain *hand*, or *direction of rotation*, towards which the old line BA may be conceived to *turn*, in the plane of the triangle (or of the paper), as indicated by the *curved arrow* in the figure.

'(7). The foregoing view, although not precisely the same with that adopted by Euclid himself, in his exposition of the elements of geometry, is at least consistent therewith; and has been made the basis of an important and modern method of calculation, respecting *directed lines in one plane*, which seems to have been first introduced about the commencement of the present century, by Argand in France, and for which Professor

De Morgan of London has lately proposed the name of *Double Algebra*: because it recognises and employs *two numerical elements* (such as the numbers 1 and 60 in the foregoing example), as required for the joint determination of the *length and direction of a straight line*. And it is now to be shown what is the nature of the passage that has been made, by the author of the Lectures on Quaternions, from such a *double system* of algebraic geometry, to what may be called, by analogy and contrast, a *quadruple system* of calculations respecting directed lines, or a system of QUADRUPLE ALGEBRA.

'(8). This passage from the one system to the other may be said to consist mainly in the consideration of the *variable plane of an angle*. If, after tracing the equilateral triangle ABC on a *card*, which at first rests on a horizontal *table*, we then lift up that card, with the figure traced thereon, and lay it on a sloping *desk*, the triangle in its new position takes also a *new aspect;* it faces a different *region* of space, and may be conceived to *look at*, or be looked at by, a *new point of the heavens*, which is *not now* the *vertical* point (or zenith), as before. This *new aspect of the figure*, or of the *plane* (or desk) on which it is now situated, is the *new circumstance* introduced, in the transition from Double to Quadruple Algebra. And in fact it is easy to see that this new circumstance, of the *varied position of the figure*, namely, of the triangle, or simply (if we choose) of the ANGLE ABC, requires the consideration of *two new numerical elements*. For we have now *two new questions* to answer, or *two new things to determine:* namely, 1st, the *slope of the desk* (or inclination of the plane), suppose forty-five degrees, conducting to a *first new number*, 45; and 2nd, the *direction of the edge* (or, technically speaking, the line of the nodes), where that slope meets the table, and which may deviate from the line of north and south by any other number of degrees, suppose seventy, giving thus a *second new number*, in this case 70.'

VOL. II., PAGE 543.

MÄDLER'S CENTRAL SUN.

Sir W. R. Hamilton's account of Mädler's attempt to determine the orbit of our own Sun in his memoir *Die Central-Sonne* (mentioned *supra*, vol. ii. p. 543, as communicated to *Saunders' Newsletter* of January 1, 1847) seems to me to possess the merit of being a very clear and effective

statement of an investigation which, even though its results have not been accepted, must always be of interest to astronomers, and I therefore reproduce it here.

'By an extensive and laborious comparison of the quantities and directions of the proper motions of the stars in various parts of the heavens, combined with indications afforded by the parallaxes hitherto determined, and with the theory of universal gravitation, Professor Mädler has arrived at the conclusion that *the Pleiades form the central group of our whole Astral or Sidereal system, including* the Milky Way and all the brighter stars, but exclusive of the *more distant nebulæ* and of the stars of which those nebulæ may be composed. And *within* this central group itself he had been led to fix on the star *Alcyone* (otherwise known by the name of Eta Tauri) as occupying exactly or nearly the position of the centre of gravity, and as entitled to be called the *Central Sun.* Assuming Bessel's parallax of the star 61 Cygni, long since remarkable for its large proper motion, to be correctly determined, Mädler proceeds to form a first approximate *estimate* of the distance of this central body from the planetary or solar system; and arrives at the (provisional) conclusion that Alcyone is about thirty four million times as far removed from us or from our own Sun as the latter luminary is from us. It would therefore, according to this estimation, be at least *a million times as distant as the new Planet,* of which the theoretical and deductive discovery has been so great and beautiful a triumph of modern Astronomy, and so striking a confirmation of the Law of Newton. The same approximate determination of distance conducts to the result that *the light of the central sun occupies more than five centuries in travelling thence to us.*

'The enormous orbit which our own Sun, with the earth and the other planets, is thus inferred to be describing about that distant centre; not indeed under *its* influence *alone,* but *by the combined attractions of all the stars which are nearer to it than we are,* and which are estimated to amount to more than one hundred and seventeen millions of masses, each equal to the total mass of our own Solar System, is supposed to require upwards of eighteen millions of years for its complete description, at the rate of about eight geographical miles in every second of time.

'The *plane* of this vast *orbit of the Sun* is judged to have an inclination of about eighty four degrees to the ecliptic, or to the plane of the annual orbit of the earth, and the longitude of the ascending *node* of the former orbit on the latter is concluded to be nearly two hundred and thirty-seven degrees.

'The general conclusions of Mädler respecting the constitution of the whole system of the fixed stars, exclusive of the distant nebulæ, are the

following:—He believes that the middle is indicated by a very rich group (the Pleiades), containing many considerable individual bodies, though at immense distances from us. Round this he supposes that there is a zone proportionally poor in stars, and then a broad, rich, ring-formed layer, followed by an interval comparatively devoid of stars, and then by another annular and starry space, and so on; the two outmost rings composing the two parts of the Milky Way, which are confounded with each other by perspective in the portions most distant from ourselves.

'Professor Mädler has acknowledged in his work his obligations, which are those of all inquirers in sidereal astronomy, to the researches of the two Herschels, Sir William and Sir John. The views of Sir William Herschel respecting the relation of our Solar System to the Milky Way will naturally recur to the recollection of our readers; and while astronomers are anxiously awaiting the shortly expected appearance of the complete account of Sir John Herschel's Observations on the Southern Nebulæ, the following passages of a letter, which was written in 1835 by that illustrious son of an illustrious sire from the Cape of Good Hope to Sir William Hamilton, may be read with peculiar interest, from the agreement between the views it expresses and some of those to which Professor Mädler has been led.

'In the letter just referred to (from which an extract was published at the time) Sir John Herschel expressed himself as follows:—

'"The general aspect of the southern circumpolar region, including in that expression 60° or 70° of S. P. D., is in a high degree bright and magnificent, owing to the superior brilliancy and larger development of the Milky Way; which, from the constellation of Orion to that of Antinous, is in a blaze of light, strangely interrupted, however, with vacant and almost starless patches, especially in Scorpio, near *a* Centauri and the Cross; while to the north it fades away pale and dim, and is in comparison hardly traceable. I think it is impossible to view this splendid zone, with the astonishingly rich and evenly distributed fringe of stars of the third and fourth magnitudes, which form a broad skirt to its southern border, like a vast curtain, without an impression amounting to a conviction, that the Milky Way is not a mere stratum, but an annulus; or, at least, that our system is placed within one of the poorer and almost vacant parts of its general mass, and that eccentrically, so as to be much nearer to the parts about the Cross than to that diametrically opposed to it."'

Vol. III., Pages 199, 200 (Note 1).

ADDITIONS TO *CONTENTS OF ELEMENTS OF QUATERNIONS.*

In editing the *Elements of Quaternions* the following addition to the *Contents*, p. lvii, was also overlooked.* It is to be inserted at the end of paragraph (*r*) and before 'Article 423—Mac Cullagh's Theorem of the Polar Plane . . .'

To paragraph (*r*) after the final words '*elastic force*' is added the word 'corresponding.' Then follow a series of paragraphs referring to sub-articles 85, 86, 87 of Article 422 in the *Elements*, pp. 754, 755.

'♀ *August* 25, 1865.

'(*s*). When a given or first ray, ρ, prolonged or shortened, becomes a second ray, ρ_1, at the same side of the centre O, so that $\mathrm{U}\rho_1 = \mathrm{U}\rho$, we can easily derive from LXIII. the expression

$$r_1 = \mathrm{T}\rho_1 = abch^{-2}, \qquad (\mathrm{T}_6)$$

or

$$r_1^2 = a^2b^2c^2\,(\mathrm{S}\rho\phi^{-1}\rho)\,; \qquad (\mathrm{U}_6)$$

so that the two quantities, h and r, are constant together or variable together: similarly for the two other quantities, h and r, which are obtained from these by interchanging *sheets*.

'(*t*). It follows, then, that one *sheet* of the *cone* (Q_6), which has its surface at the centre of the wave, and rests on a sphero-conic (r_1) traced on the wave-sheet, contains also, or may be considered as likewise *resting upon*, a *line of vibration* (h) on the *other sheet*, and reciprocally; so that *each* of these two curves is *projected* into the other, by rays from *O*, and one would appear as *superposed* on the other, if we imagine them to be *seen* by an eye placed at that point. As a limiting case, when the *projecting cone* reduces itself to one of the two *principal planes*—for example, to the plane (*a*)—then the ellipse (*a*) in that plane may be represented by the equation $h^2 = bc$, and the circle (*a*) has for equation $r_1 = a$; so that the condition (T_6) is satisfied.

'♄ *August* 26, 1865.

'(*u*). In fact the *quadric cone* (Q_6) must cut the *quartic wave* in an *octic curve*, or else in a *system* of curves, of which the product of the

* See Manuscript-Book D., 1864, pp. 32, 33; and 41 . . . 50. See also A. 1865, pp. 101, 111, 151.

dimensions is *eight;* and accordingly we find, as above, that the *complete* intersection, here considered, of the two surfaces, consists of a system of two *quartic curves*, namely, a *sphero-conic* (r_1) on *one sheet*, and a *line of vibration* (h) on the other.

'(*v*). [In the manuscript-book nothing follows after this "(*v*)."]'

The above date, 'August 26, 1865,' is the last on record, I believe, in Sir W. R. H.'s manuscript-books, of scientific *work*. The paragraphs here given have not previously been printed. It is to be noted that the author died on the 2nd of September, 1865.

I find in Manuscript-Book C, 1864, p. 73, the following important notes, communicated to Professor Tait, on earlier pages of the *Contents* of *Elements of Quaternions* :—

At foot of p. li. :—'☿ April 26, '65.—I believe that it is *better* to multiply the mass into the *half-square* of the (scalar) velocity, and to call *this* product the *Living Force*, as recommended by some modern writers; but as I was not treating of *Physics* proper, I have been content to use here the older designation.—W. R. H.'

At foot of p. liii. :—'The $r + 1$ elongations of the disturbing forces in the rth group, are

$$r\theta,\ (r-2)\theta,\ (r-4)\theta,\ \ldots\ \ (4-r)\theta,\ (2-r)\theta,\ -r\theta.'$$

At foot of p. lv. :—'April 26, '65.—I have scarcely at all looked lately into the memoranda of our correspondence of 1858–59, but my impression is that the *old* v was the new $-v$. The new v and ω have simpler geometrical significations.'

In connexion with what has been stated, *supra*, p. 124 (note), I have to report that I have since come upon passages of a Journal for 1861, which leave no doubt as to the intention of the author of the *Elements* to add Note A on *Anharmonic Co-ordinates*, and Note B upon the *Barycentric Calculus*. From these passages we learn also more fully his intention as to a Note C (indicated in the *Elements*, p. 296 (note)), as well as with reference to an Index. The passages are as follow :—

'*August* 26, 1861. No. 444.—My present intention is to confine the few remaining pages of the present [Manuscript] Book to memoranda connected with the printing of Book II. of the *Elements;* which will scarcely end before the 300*th printed page*, if it do not extend beyond that limit. [It ended *exactly* with the 300*th* page.— W. R. H.] Consequently it seems

even more likely than before (compare No. 409), that I may be content to conclnde the VOLUME (of 1861)* at the end of that *Second Book*, since it will be absolutely necessary to prefix *some Preface* and *some Table of Contents*, as also to print *some Index*, for which indeed W. E. H. has begun to make *preparations:* and I have already promised *at least two Notes*, A and B (*Elements*, pages 34, 35, &c.) For I should not like to go *much beyond* 300 printed pages.'

'*August* 28, No. 453.—Have searched for and collected *References to Future Notes*, which seem as yet to be only A and B (see No. 444), though I thought there had been a promise of a Note C; [There *is* such a promise, or allusion, in the note to Art. 272 in page 296, February 11, 1862. —W. R. H.], and now think that I shall perhaps insert one, containing some account of the new Geometrical Theorems stated in *Nichol's Cyclopædia*.'

'*August* 13, 1861. No. 409.—I remarked [to the printer] that Chap. III. was likely to be much shorter; and that *perhaps* I might decide on publishing the *Two First Books*, with some Preface, &c., as a FIRST VOLUME.'

ERRATA IN 'LECTURES ON QUATERNIONS' AND IN 'ELEMENTS OF QUATERNIONS.'

The possessors of the *Lectures on Quaternions* and *Elements of Quaternions* may also be glad to be furnished with corrections of errors, which I have found noted in Hamilton's Manuscript-Books.

'LECTURES ON QUATERNIONS.'

In A. 1857, p. 509, the following corrections are given as to be added to the Errata of the *Lectures*:—p. 686, the formulæ [27] and [28] ought to be printed thus—

$$[27] \qquad 1 = f(\rho) = g\rho^2 + 2\Sigma \mathrm{S} \,.\, \alpha\rho . \mathrm{S} \,.\, \beta\rho + 2\mathrm{S} \,.\, \gamma\rho,$$

(the last coefficient, 2, having been omitted).

$$[28] \quad \ldots \nu = \phi(\rho) = \{g\rho + \Sigma(\alpha \mathrm{S} \,.\, \beta\rho + \beta \mathrm{S} \,.\, \alpha\rho) + \gamma\} \, (1 - \mathrm{S} \,.\, \gamma\rho)^{-1}:$$

'ELEMENTS OF QUATERNIONS.'

In A. 1865, p. 101, two important errata, in connexion with fig. 50, p. 190, of the *Elements*, are noted:—*instead of* $\beta' \| \beta$ *read* $\beta' \| \alpha$; *instead of* $\beta'' \perp \beta$ *read* $\beta'' \perp \alpha$.

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* Compare *supra*, p. 133.

Vol. III., Page 200 (Note 2).

MEMOIRS IN WHICH DETERMINANTS ARE USED.

With the date September 6, 1864 (E. 1864. Div. I. § 2, page 11), a treatise, incomplete, though occupying 30 folio pages and numbering 41 articles (the 41st article being unfinished), has for its subject the following problem :—

'Let x, y, z be the co-ordinates, right or oblique, of a point P of a curve in space or any linear and homogeneous functions of such co-ordinates; and let $x'\ y'\ z'$, $x''\ y''\ z''$, . . . and $x^v\ y^v\ z^v$ be their first, second . . . and fifth derivatives, taken with respect to any independent variable, t. Let also $X + x$, $Y + y$, $Z + z$, be the co-ordinates (in the same system) of any other point Q of space. It is required to form, by determinants, and without quaternions, the equation in XYZ of the *quadric cone*, which has its vertex at P, and has *five-side contact* with the *cone of chords* from that point, drawn to other points of the curve : $x'\ y'\ z'$ . . . $x^v\ y^v\ z^v$ being here supposed to be given.'

A second treatise, dated September 8, 1864 (E. 1864, Div. III. § 1, page 101), has for its title 'Extracts and Remarks from and on Dr. Salmon's *Lessons Introductory to the Modern Higher Algebra*, Dublin, 1859.' It occupies 17 folio pages, the last being headed: 'March 24, 1865.— Example of Solution of a System of three Equations.' Of this the last article, 34, is incomplete.

The following page (E. 1864, p. 119) has the very late date, ♄ July 8, 1865, and is headed *Elimination by Determinants;* of which it gives three examples.

Vol. II., Page 103.

KANT'S PHILOSOPHY.

The following passage, on the subject of Kant's Philosophy, is reproduced from a fragment of a draft of a letter to Mr. De Vere, bearing a date only a few days anterior to that of the admirable letter on the same subject, which is printed in Vol. II., pp. 103–5 of this work. I regret that it was not introduced in its proper place, and I insert it here, as it

seems to me to set forth with great clearness fundamental principles compendiously referred to in that letter.

'Observatory, August 8, 1834 . . . Meanwhile I think I can form a better notion than when I last wrote about the object of Kant's "Practical Reason"—for though I have not yet had the pleasure of reading or even seeing that work, I have set myself seriously to the task of reviewing my almost forgotten knowledge of German, and have studied a large part of the Critique of the "Pure Reason" in the original with a pleasure which I wish that I could impart to you. Perhaps indeed you too may have lately read the work; but if not, it may interest you to receive a short sketch of its purpose and results so far as I know, and can in a letter state them. Kant begins by distinguishing between *priori* and *posteriori* knowledge; the latter having its source in experience, the former not, although *in the order of time* no knowledge is before experience; and among knowledges of the priori kind, he calls those *pure* which contain in them nothing empirical. As marks by which priori may be distinguished from empirical knowledge he mentions Necessity and Universality; experience never showing that a thing *must* be, nor even (in a strict sense) that it *always* is; and he establishes by these marks the existence of priori knowledges, not only in mathematics but in other regions of thought. Now judgments generally, he shows, are either Analytic or Synthetic; they either analyse and *illustrate* our former knowledge, or by a synthesis they add to and enlarge it. In all theoretical sciences of the Reason, synthetic judgments *à priori* are contained as principles; and it is, according to Kant, the general or fundamental *Problem of the Pure Reason*, How are such judgments possible? How can we gain new knowledge, or effect any synthesis of thought in the pure priori sphere—not deducing, on the one hand, by analysis, nor gathering, on the other hand, from experience—and since analysis does [here ends the manuscript.]

LIST

OF

PAPERS, MEMOIRS, ADDRESSES, AND BOOKS

PUBLISHED BY

SIR WILLIAM ROWAN HAMILTON,

AND OF

NOTICES OF COMMUNICATIONS.

An Asterisk is prefixed to items not contained in the Royal Society's Catalogue.

*Review of two scientific memoirs of James MacCullagh, B.A.: *National Magazine*, vol. I., pp. 145–149. Dublin, August, 1830.

Theory of Systems of Rays [1824]: *Irish Academy Transactions*, XV., 1828, pp. 69–174; XVI., 1830, pp. 4–62; XVI., Part II., 1831, pp. 93–125; XVII., 1837, pp. 1–144.

[In this XVIIth volume is published Hamilton's "Third Supplement," containing "Conical Refraction." Professor Lloyd's accounts of the experiments by which he verified Hamilton's results are contained in the numbers of the *Philosophical Magazine* for February and March 1832, pp. 112 and 207, and also in the XVIIth volume (pp. 145–157) of the *Irish Academy Transactions*, above mentioned. A Paper on *Caustics* had been presented to the R. I. A., December 13, 1824: and this Paper, when re-cast, was called the *Theory of Systems of Rays* (*supra* vol. i., pp. 176 *and* 187).—R. P. G.]

On the Error of a received principle of Analysis, respecting Functions which vanish with their Variables: *Irish Academy Transactions*, XVI., 1830, pp. 63, 64; Part II., 1831, pp. 129, 130.

On a view of mathematical Optics: *British Association Report*, 1831–2, pp. 545–547.

*Introductory Lecture on Astronomy: *Dublin University Review*, January 1833, pp. 72–85.

*Review of Arago's work, "*The Comet*, translated by Colonel Charles Goold, London, 1833": *Dublin University Review*, April 1833, p. 365. (Two slight contributions entitled *The Comet* will be found in the *Dublin Penny Journal*, December 1832, p. 207, 208, and January 1833, pp. 223, 224.")

On some Results of the View of a Characteristic Function in Optics: *British Association Report*, 1833, pp. 360–370.

*On a New Method of investigating the relations of Surfaces to their Normals, with results respecting the Curvatures of Ellipsoids: *Dublin University Review*, July 1833, pp. 653–4.

On the Effect of Aberration in prismatic Interference: *Philosophical Magazine*, II., 1833, pp. 191–194; Poggendorff's *Annalen der Physik und Chemie*, XXIX., 1833, pp. 316–318.

On the undulatory Time of Passage of Light through a Prism: *Philosophical Magazine*, II., 1833, pp. 284–287; Poggendorff's *Annalen der Physik und Chemie*, XXIX., 1833, pp. 323–327.

Note on Mr. POTTER'S Reply respecting his experiment of prismatic Interference: *Philosophical Magazine*, II., 1833, p. 371; Poggendorff's *Annalen der Physik und Chemie*, XXIX., 1833, pp. 328, 329.

*On a General Method of expressing the Paths of Light and of the Planets, by the Co-efficients of a Characteristic Function: *Dublin University Review*, November 1833, pp. 795–826.

On the Application to Dynamics of a General Mathematical Method previously applied to Optics: *British Association Report*, 1834, pp. 513–518.

On Conjugate Functions, or Algebraic Couples, as tending to illustrate generally the doctrine of Imaginary Quantities, and as confirming the results of Mr. [J. T.] GRAVES respecting the existence of two independent Integers in the complete expression of an Imaginary Logarithm: *British Association Report*, 1834, pp. 519–523.

ON A GENERAL METHOD IN DYNAMICS, by which the study of the motions of all free systems of attracting or repelling points is reduced to the search and differentiation of one central Relation or characteristic Function: *Philosophical Transactions of the Royal Society*, 1834, pp. 247–308; Quetelet's *Correspondance Mathématique*, VIII., 1834, pp. 69–89, 200–211.

Remarques sur un mémoire de M. PLANA, Tome VII. [sur les rayons réfractés]: Quetelet's *Correspondance Mathématique*. VIII., 1834, pp. 27–30.

*Address as Secretary of the Dublin Meeting of the British Association: *British Association Report*, 1835, pp. xli.–lvi.; *also*, Philip Dixon Hardy's *Proceedings of the Fifth Meeting of the British Association for the Advancement of Science*, Dublin, 1835, pp. 28–34.

*On a new theory of Logologues; *also*, On a new theory of varying Orbits: *British Association Report*, 1835, Part II., p. 7.

SECOND ESSAY ON A GENERAL METHOD IN DYNAMICS: *Philosophical Transactions of the Royal Society*, 1835, pp. 95–144.

Inquiry into the Validity of a Method recently proposed by G. B. JERRARD, Esq., for Transforming and Resolving Equations of Elevated Degrees: *British Association Report*, 1836, pp. 295–348.

Calculus of Principal Relations: *British Association Report*, 1836, Part II., pp. 41–44.

Theorem connected with the Question of resolving in finite Terms the Equation of the Fifth Degree: *Philosophical Magazine*, VIII., 1836, pp. 538–543; and IX., 1836, pp. 28–32.

On Differences and Differentials of functions of Zero [1831]: *Irish Academy Transactions*, XVII., 1837, pp. 235, 236; Quetelet's *Correspondance Mathématique*, VIII., 1834, pp. 235–7.

*Exposition of the Argument of ABEL: *British Association Report*, 1837, Part II., p. 1.

*New Applications of the Calculus of Principal Relations; and Exposition of Mr. TURNER's theorem of odd numbers, &c.: *British Association Report*, 1837, Part II., p. 1.

Theory of CONJUGATE FUNCTIONS or Algebraic Couples, with a preliminary and elementary Essay on ALGEBRA AS THE SCIENCE OF PURE TIME [1835]: *Irish Academy Transactions*, XVII., 1837, pp. 293–422.

On the Propagation of Light *in vacuo*: *British Association Report*, 1838, Part II., pp. 2–6.

On the Propagation of Light in Crystals: *British Association Report*, 1838, Part II., p. 6.

On the Argument of ABEL, respecting the impossibility of expressing a root of any General Equation above the Fourth Degree by any finite combination of Radicals and Rational Functions [1837]: *Irish Academy Transactions*, XVIII., 1839, pp. 171–259.

Investigations respecting Equations of the Fifth Degree [May 22, 1837]: *Irish Academy Proceedings*, I., 1841, pp. 76–80.

*Inaugural Address as President of the Royal Irish Academy [January 8, 1838]: *Irish Academy Proceedings*, I., 1841, pp. 107–120.

*Address as President of the Royal Irish Academy on Professor MAC CULLAGH's Paper on the Laws of Crystalline Reflexion and Refraction [June 25, 1838]: *Irish Academy Proceedings*, I., 1841, pp. 212–221.

On the Dynamics of Light [*communicated* January 14, *and* February 11, 1839]: *Irish Academy Proceedings*, I., 1841, pp. 245, 267–270.

Notice of a singular appearance of the clouds observed on the 16th of December, 1838 [*communicated* January 14, 1839]: *Irish Academy Proceedings*, I., 1841, p. 249.

*Address as President of the Royal Irish Academy on Dr. APJOHN's researches on the Specific Heats of Gases [February 25, 1839]: *Irish Academy Proceedings*, I., 1841, pp. 276–284.

Researches respecting Vibration, connected with the Theory of Light [*communicated* June 24, 1839]: *Irish Academy Proceedings*, I., 1841, pp. 341–349.

*Address as President of the Royal Irish Academy on Mr. PETRIE's Paper on the History and Antiquities of Tara Hill [June 24, 1839]: *Irish Academy Proceedings*, I., 1841, pp. 350–354.

On FLUCTUATING FUNCTIONS [*communicated* June 22, 1840]: *Irish Academy Proceedings*, I., 1841, pp. 475–477; *Irish Academy Transactions*, XIX. 1843, pp. 264–321.

**See also*: Supplementary Remarks on Fluctuating Functions [*communicated* February 28, 1842]: *Irish Academy Proceedings*, II., 1844, pp. 232–238.

On a Mode of deducing the Equation of FRESNEL's Wave: *Philosophical Magazine*, XIX., 1841, pp. 381–3.

*New Demonstration of FOURIER's theorem [*communicated* June 28, 1841]: *Irish Academy Proceedings*, II., 1844, p. 129.

On the Focal Lengths and Aberrations of a thin Lens of Uni-axal Crystal, bounded by Surfaces which are of Revolution about its Axis: *Philosophical Magazine*, XIX., 1841, pp. 289–294.

On certain discontinuous Integrals connected with the Development of the Radical which represents the Reciprocal of the Distance between two Points: *Philosophical Magazine*, XX., 1842, pp. 288–294.

*On a mode of expressing Fluctuating or Arbitrary Functions by mathematical formulæ: *British Association Report*, 1842, Part II., p. 10.

On a Theorem in the Calculus of Differences: *British Association Report*, 1843, Part II., pp. 2, 3.

On some investigations connected with the Calculus of Probabilities: *British Association Report*, 1843, Part II., pp. 3, 4.

On Equations of the Fifth Degree; and especially on a certain System of Expressions connected with those Equations which Professor BADANO has recently proposed [1842]: *Irish Academy Transactions*, XIX., 1843, pp. 329–376.

* *See also communications of* August 4, 1842, *and of* February 27, 1843: *Irish Academy Proceedings*, II., pp. 275, 276, *and* p. 355.

On an Expression for the Numbers of BERNOULLI by means of a Definite Integral, and on some connected Processes of Summation and Integration: *Philosophical Magazine*, XXIII., 1843, pp. 360–367.

On the Composition of Forces [*communicated* Nov. 8, 1841]: *Irish Academy Proceedings*, II., 1844, pp. 166–170.

On the Day of the Vernal Equinox at the time of the Council of Nice [*communicated* May 9, 1842]: *Irish Academy Proceedings*, II., 1844, pp. 249, 250.

On Dr Robinson's Table of Mean Refractions [*communicated* May 22, 1843]: *Irish Academy Proceedings*, II., 1844, pp, 400, 401.

*Address as President of the Royal Irish Academy, on Dr. Kane's Researches on the Nature of Ammonia [June 26, 1843]: *Irish Academy Proceedings*, II., 1844, pp. 412–419.

On the Calculus of Probabilities [*communicated* July 31, 1843]: *Irish Academy Proceedings*, II., 1844, pp. 420–422.

*On a new species of Imaginary Quantities connected with the Theory of Quaternions [*communicated* November 13, 1843]: *Irish Academy Proceedings*, II., 1844, pp. 424–434.

[*See* Life, vol. ii., pp. 443, 444. The intimation to the Academy of the discovery had been made on October 16, 1843].

*On approximating to the calculation of Eclipses [*communicated* May 27, 1844]: *Irish Academy Proceedings*, II., 1844, p. 597.

*On Quaternions [November 11, 1844]: *Irish Academy Proceedings*, III., 1847, pp. 1–16.

*On Quaternions [June 23, 1845]: *IrishAcademy Proceedings*, III., 1847, p. 109.

*On Quaternions [July 20, 1846]: *Irish Academy Proceedings*, III., 1847, pp. 273–292.

On Quaternions: *Philosophical Magazine*, XXV., 1844, pp. 10–13, 241–6, 489–95; XXVI., 1845, pp. 220–24; XXIX., 1846, pp. 26–31, 113–122, 326–8; XXX., 1847, pp. 458–461; XXXI., 1847, pp. 214–219, 278–293, 511–519; XXXII., 1848, pp. 367–374; XXXIII., 1848, pp. 58–60; XXXIV., 1849, pp. 294–297, 340–343, 425–439; XXXV., 1849, pp. 133–137, 200–204; XXXVI., 1850, pp. 305, 306.

On Quaternions: *British Association Report*, 1844, Part II., p. 2; 1845, Part II., p. 3.

Exercises in Quaternions: *Cambridge and Dublin Mathematical Journal*, IV., 1849, pp. 161–168.

Researches respecting Quaternions, First Series: *Irish Academy Transactions*, XXI., 1848, pp. 199–296. [*See* Life, Vol. II., p. 444.]

Sur les Quaternions: *Nouvelles Annales de Mathématiques*, XII., 1853, pp. 275–283.

On Symbolical Geometry: *Cambridge and Dublin Mathematical Journal*, I., 1846, pp, 45–57, 137–154, 256–263; II., 1847, pp. 47–52, 130–133, 204–209; III., 1848, pp. 68–84, 220–225; IV., 1849, pp. 84–89, 105–118.

On two Theorems of Central Forces [*communicated* November 30, 1846]: *Irish Academy Proceedings*, III., 1847, pp. 308, 309.

A new method of expressing in symbolical language the Newtonian Law of Attraction [the Law of the Circular Hodograph; *communicated* December 14, 1846]: *Irish Academy Proceedings*, III., 1847, pp. 344–353.

On a Theorem of Hodographic Isochronism [*communicated* March 16, 1847, *and* May 10, 1847]: *Irish Academy Proceedings*, III., 1847, pp. 417, 465, 466.

On the application of the Calculus of Quaternions to the Theory of the Moon [*communicated* June 14, 1847]: *Irish Academy Proceedings*, III., 1847, pp. 507–520.

Illustrations from Geometry of the Theory of Algebraic Quaternions [*communicated* February 10, 1845]: *Irish Academy Proceedings*, III., 1847, *Appendix*, pp. xxxii–xxxvi.

* [Note on p. xxxi of the *Appendix* to Vol. III., 1847, of the *Irish Academy Proceedings*:—'The Abstract of Sir W. R. Hamilton's Memoir on Quaternions, read on February 10, 1845, and referred to in page 64, has not been received for insertion in this Appendix.

'Nor have the Abstracts of Sir W. R. Hamilton's Memoirs on the New Imaginaries, read on July 14 and 21, 1845, and referred to in pages 111, 112 been received.'

They were printed for private circulation. Copies are deposited with the Hamilton Papers in the Library of Trinity College, Dublin.—R. P. G.].

On the application of the Method of Quaternions to some Dynamical questions [*communicated* July 14, *and* July 21, 1845]: *Irish Academy Proceedings*, III., 1847, *Appendix*, xxxvi–l.

*Additional applications of the Theory of Algebraic Quaternions [*communicated* December 8, 1845]: *Irish Academy Proceedings*, III., 1847, *Appendix*, pp. li–lx.

*On an Isoperimetrical Problem treated by the Calculus of Quaternions: *British Association Report*, 1847, Part II., p. 4.

*On some applications of the Calculus of Quaternions to the Theory of the Moon: *British Association Report*, 1847, Part II., p. 4.

*On additional applications of Quaternions to Surfaces of the Second Order [*communicated* November 30, 1847]: *Irish Academy Proceedings*, IV., 1850, pp. 14–19.

*On Quaternions and the Rotation of a Solid Body [*communicated* January 10, 1848]: *Irish Academy Proceedings*, IV., 1850, pp. 38–56.

*On Quaternions and the determination of the Distances of any recently discovered Comet or Planet [*communicated* February 28, 1848]: *Irish Academy Proceedings*, IV., 1850, p. 75.

*On the New Planet *Metis* [*communicated* May 22, 1848]: *Irish Academy Proceedings*, IV., 1850, p. 169.

On the Double Mode of Generation of an Ellipsoid [*communicated* May 22, 1848]: *Irish Academy Proceedings*, IV., 1850, p. 173.

Additional theorems respecting certain reciprocal Surfaces [*communicated* June 26, 1848]: *Irish Academy Proceedings*, IV., 1850, pp. 192, 193.

*On Quaternions applied to Problems respecting the construction of a Circle touching three given Circles on a Sphere, and of a Sphere touching four given Spheres [*communicated* December 11, 1848]: *Irish Academy Proceedings*, IV., 1850, p. 255.

On Theorems relating to Surfaces, obtained by the method of Quaternions [*communicated* February 26, 1849]: *Irish Academy Proceedings*, IV., 1850, pp. 306–308.

On an Equation of the Ellipsoid [*communicated* April 9, 1849]: *Irish Academy Proceedings*, IV., 1850, pp. 324, 325.

On the inscription of certain "gauche" Polygons in Surfaces of the Second Degree [*communicated* April 9, 1849]: *Irish Academy Proceedings*, IV., 1850, pp. 325–6.

On the construction of the Ellipsoid by two sliding spheres [*communicated* April 23, 1849]: *Irish Academy Proceedings*, IV., 1850, pp. 341, 342.

On a Theorem respecting Ellipsoids, obtained by the method of Quaternions [*communicated* May 28, 1849]: *Irish Academy Proceedings*, IV., 1850, pp. 349–355.

On some results obtained by the Quaternion analysis respecting the inscription of 'gauche' Polygons in Surfaces of the Second Order [*communicated* June 25, 1849]: *Irish Academy Proceedings*, IV., 1850, pp. 380–387.

*On some new applications of Quaternions to Geometry: *British Association Report*, 1849, Part II., p. 1.

On "gauche" Polygons in central Surfaces of the Second Order [*communicated* May 13, 1850]: *Irish Academy Proceedings*, IV., 1850, pp. 541–556.

*On Polygons inscribed on a Surface of the Second Order: *British Association Report*, 1850, Part II., p. 2.

On Continued Fractions in Quaternions: *Philosophical Magazine*, III., 1852, pp. 371–373; IV., 1852, p. 303; V., 1853, pp. 117, 118, 236–238, 321–326.

*On a Proof from Quaternions of the celebrated Theorem of JOACHIMSTHAL [*communicated* January 27, 1851]: *Irish Academy Proceedings*, V., 71.

A generalization of PASCAL's Theorem [*communicated* March 16, 1851]: *Irish Academy Proceedings*, V., 1853, pp. 100, 101.

On the nature and properties of the Aconic Function of six Vectors [*communicated* June 23, 1851]: *Irish Academy Proceedings*, V., 1853, pp. 177–186.

On the connexion of Quaternions with Continued Fractions, and Quadratic Equations [*communicated* December 8, 1851, *and* May 24, 1852]: *Irish Academy Proceedings*, v., 1853, pp. 219–221, 299–301.

*On Biquaternions: *British Association Report*, 1852, Part II., p. 2.

On the Geometrical Interpretation of some results obtained by calculation with Biquaternions [February 28, 1853]: *Irish Academy Proceedings*, v., 1853, pp. 388–390.

On the Geometrical Demonstration of some theorems by means of the Quaternion analysis [*communicated* April 11, 1853]: *Irish Academy Proceedings*, v., 1853, pp. 407–415.

*Theorem concerning Polygonic *Syngraphy* [*communicated* June 13, 1853]: *Irish Academy Proceedings*, v., 1853, pp. 474, 475.

*Lectures on Quaternions: Dublin, Hodges and Smith, 1853, pp. (64), lxxii., 736. [*Supra* Vol. ii., pp. 679–685.]

*On the Integrations of certain Equations [*communicated* February 27, 1854]: *Irish Academy Proceedings*, vi., 1858, pp. 62, 63.

On the celebrated Theorem of Dupin [*communicated* May 8, 1854]: *Irish Academy Proceedings*, vi., 1858, pp. 86–88.

On some Extensions of Quaternions [*communicated* June 26, 1854]: *Irish Academy Proceedings*, vi., 1858, pp. 114, 115.

On some Extensions of Quaternions: *Philosophical Magazine*, vii., 1854, pp. 492–499; viii., 1854, pp. 125–137, 261–269; ix., 1855, pp. 46–51, 280–290.

*On an Extension of Quaternions: *British Association Report*, 1854, Part II., p. 1.

On the Solution of the Equation of Laplace's Functions [*communicated* February 26, 1855]: *Irish Academy Proceedings*, vi., pp. 181–185.

*Symbolical Extensions of Quaternions; *and* Geometrical Applications of Quaternions [*communicated* June 11, 1855]: *Irish Academy Proceedings*, vi., 1858, p. 250, 260, 311.

*On the conception of the Anharmonic Quaternion, and on its application to the Theory of Involution in Space: *British Association Report*, 1855, Part II., p. 7.

Memorandum respecting a new System of Roots of Unity [The Icosian Calculus]: *Philosophical Magazine*, xii., 1856, p. 446.

*Account of the Icosian Calculus [*communicated* November 10, 1856]: *Irish Academy Proceedings*, vi., 1858, pp. 415, 416. [*See also Irish Academy Proceedings* [February 9, 1857], vi., 1858, p. 462.]

*On a General Expression by Quaternions for Cones of the Third Order [*communicated* May 11, 1857]: *Irish Academy Proceedings*, vi., 1858, p. 506. *See also Irish Academy Proceedings* [May 25, 1857], vi., 1858, p. 512.

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On some Quaternion Equations connected with FRESNEL's wave surface for bi-axal crystals [*communicated* February 28, 1859, *and* May 9, 1859]: *Irish Academy Proceedings*, VII., 1862, pp. 122–124, 163.

On some Quaternion Equations connected with FRESNEL's wave-surface for bi-axal crystals: *British Association Report*, 1859, Part II., p. 248.

On some Quaternion Equations connected with FRESNEL's wave-surface for bi-axal crystals: *Natural History Review*, VI., 1859, pp. 240–242, 365.

* [The *Natural History Review and Quarterly Journal of Science* was published under the editorship of W. H. Harvey and Samuel Haughton, Vol. i.–vii. London and Dublin, 1854–1860.]

*On ANHARMONIC CO-ORDINATES [*communicated* April 9, 1860; May 28, 1860; June 25, 1860]: *Irish Academy Proceedings*, VII., 1862, pp. 286–289, 329, 350–354.

On Anharmonic Co-Ordinates: *Natural History Review*, VII., 1860, pp. 242–246, 325–327, 506–509.

*On GEOMETRICAL NETS IN SPACE [*communicated* June 24, 1861]: *Irish Academy Proceedings*, VII., 1862, p. 532–582.

*On Geometrical Nets in Space: *British Association Report*, 1861, Part II., p. 4.

Quaternion Proof of a Theorem of Reciprocity of Curves in Space: *British Association Report*, 1862, Part II., p. 4.

Elementary Proof that Eight Perimeters of the Regular inscribed Polygon of Twenty Sides exceed Twenty-five Diameters of the Circle: *Philosophical Magazine*, XXIII., 1862, pp. 267–269.

On a New and General Method of Inverting a Linear and Quaternion Function of a Quaternion [*communicated* June 9, 1862]: *Irish Academy Proceedings*, VIII., 1864, pp. 182, 183.

On the Existence of a Symbolic and Biquadratic Equation, which is satisfied by the Symbol of Linear Operation in Quaternions [*communicated* June 23, 1862]: *Irish Academy Proceedings*, VIII., 1864, pp. 190, 191.

On the Existence of a Symbolic and Biquadratic Equation, which is satisfied by the Symbol of Linear or Distributive Operation on a Quaternion: *Philosophical Magazine*, XXIV., 1862, pp. 127, 128.

On "Gauche" Curves of the Third Degree [*communicated* April 27, 1863]: *Irish Academy Proceedings*, VIII., 1864, pp. 331–334.

On a General Centre of Applied Forces [*communicated* (May 25), June 22, 1863]: *Irish Academy Proceedings*, VIII., 1864, p. 394.

*On the Locus of the Osculating Circle to a Curve in Space [*communicated* June 22, 1863]: *Irish Academy Proceedings*, VIII., 1864, p. 394.

*On the Eight Imaginary Umbilical Generatrices of a Central Surface of the Second Order [*communicated* January 11, 1864]: *Irish Academy Proceedings*, VIII., 1864, p. 471.

On RÖBER'S Construction of the Heptagon: *Philosophical Magazine*, 4th Series, XXVII., February 1864, pp. 124–132.

*NOTE, *appended to a Paper by the Rev. Charles Graves;* On a Theorem relating to the Binomial Co-efficients [*communicated* June 26, 1865]: *Irish Academy Proceedings*, IX., 1867, pp. 297–302.

On a New System of Two General Equations of Curvature, including as easy consequences a new form of the Joint Differential Equation of the Two Lines of Curvature, with a new Proof of their General Rectangularity; and also a new Quadratic for the Joint Determination of the Two Radii of Curvature: all deduced by GAUSS'S Second Method, for discussing generally the Properties of a Surface; and the latter being verified by a Comparison of Expressions for what is called by him the Measure of Curvature [*communicated* June 26, 1865]: *Irish Academy Proceedings*, IX., 1867, pp. 302–305.

POSTHUMOUS PUBLICATIONS.

ELEMENTS OF QUATERNIONS. London: Longmans, Green, & Co., Octavo, pp. i.–lix, 1-762.

[ELEMENTE DER QUATERNIONEN. Deutsch von DR. PAUL GLAN, Docent für Physik an der Universität Berlin. Zwei Bände. Johann Ambrosius Barth, Leipzig, 1882–84. I Band, pp. xxiv, 746, gr. 8°. (Theorie der Qu.); II Band, pp. lxxiii, 436, gr. 8°. (Anwendungen.)]

On the Elementary Conceptions of Mathematics. Seven Letters to Viscount Adare (March and April, 1835): *Hermathena*, Vol. III., pp. 469–489. Dublin, 1883.

Remarks, chiefly Astronomical, on what is known as the Problem of Hipparchus [1855]: *Hermathena*, Vol. IV., pp. 480–506. Dublin, 1883.

As an Addendum to the foregoing List, it may here be stated that there remain in manuscript the following Papers by Sir W. R. Hamilton, considerable both for their extent and importance; and other mathematical investigations of value may probably be found in his numerous Manuscript-books. All these remains are, or shortly will be, deposited for reference in the Manuscript Room of the Library of Trinity College, Dublin:—

1. A criticism on the work by Scheffler, entitled *Der Situations-Kalkul.* 1856.
2. An extension by means of Quaternions of some propositions laid down by Gauss in his *Disquisitiones Arithmeticæ.* 1856. As to Nos. 1 and 2, *vid. supra*, p. 52.
3. Two Letters to Professor De Morgan on Multiple and Definite Integrals. 96 folio pages. 1858. *Vid. supra*, p. 95.
4. Letter (with Postscript) to Andrew S. Hart, LL.D., S.F.T.C.D., on Anharmonic Co-ordinates. 280 folio pages. 1864. *Vid. supra*, p. 123, also Note to p. 124.
5. '*Lines of Curvature and Curvatures of Surfaces, partly by Quaternions, partly by the Methods of Monge and Dupin.*' 38 pages, 130 articles. 1864. *Vid. supra*, p. 201.
6. '*Gauss's Measure of Curvature of a Surface.*' 2 pages, 11 articles. 1864. *Vid. supra*, p. 201.
7. '*Intersections of Normals to Quadrics.*' 74 pages, 262 articles, 1864. *Vid.* p. *supra*, 201.
8. '*Locus of the Vertex of a Quadric Cone having Six-point contact with a Curve in Space.*' 48 pages, 224 articles. *Vid supra.* p. 201.

I transfer from the conclusion of Professor Tait's article *Quaternions*, in the ninth Edition of the *Encyclopædia Britannica*, the following list of works on the subject:—

Allegret: *Essai sur le calcul des Quaternions.* Paris, 1862.

Tait: *An Elementary Treatise on Quaternions.* Oxford, 1867; second edition, 1873; a German translation by von Scherff, 1880, and a French translation by Plarr, 1882-1884.

Kelland and Tait: *Introduction to Quaternions.* London, 1873; second edition, 1882.

Hoüel: *Eléments de la théorie des Quaternions.* Paris, 1874.

K. W. Unverzagt: *Theorie der goniometrischen und longimetrischen Quaternionen.* Wiesbaden, 1876. 312 pp,

Laisant: *Introduction à la méthode des Quaternions.* Paris, 1881.

Graefe: *Vorlesungen über die Theorie der Quaternionen.*

Dillner (in Upsala): *Versuch einer neuen Entwicklung der Hamilton'schen Methode, genannt "Calculus der Quaternionen."* (An article in the *Mathematische Annalen*, XI. Band. Leipzig, 1877).

Peirce (Professor Benjamin): *Linear Associative Algebra* (*American Journal of Mathematics*, Vol. IV., 1881).

[The author of this Paper in a *note*, p. 105, makes objection to Quaternions on the ground of the treatment of imaginaries. A reply to this objection may, I believe, be gathered from what will be found stated by Sir William Hamilton in pages 578, 579 of Vol. II., and pages 84, 85 of Vol. III. of this work, as well as *passim* in the Correspondence with Professor De Morgan.—R. P. G.]

To this list Prof. Tait adds:—

"Sylvester and others have recently published extensive contributions to the subject, including quaternions under the general class matrix, and have developed much farther than Hamilton lived to do the solution of equations in quaternions."

Without any pretension to rendering complete the above bibliography of the subject, I here set down some further references as likely to be useful to the student:—

Mathematische Annalen, XII., 1877, pp. 375–386:
Hermann Grassmann: *Der Ort der Hamilton'schen Quaternionen in der Ausdehnungslehre.*

Zeitschrift für Mathematik und Physik, Teubner, Leipzig, criticises certain works on Quaternions in the *Literaturzeitung* at the end of each volume as follows:—

XII. Band, 1867, p. 61: Hankel, *Vorlesungen über die complexen Zahlen, und ihre Functionen;* I. Theil: *Theorie der complexen Zahlensysteme.* Leipzig, 1861.

XXII. Band, 1877, p. 83 of the *Historisch-literarische Abtheilung:* Unverzagt's Work mentioned above by Professor Tait.

XXIII. Band, 1878, p. 191 of the *Historisch-literarische Abtheilung:* Unverzagt: *Der Winkel als Grundlage mathematischer Untersuchungen*, 1877–8.

XXIV. Band, 1879, p. 197 of the *Historisch-literarische Abtheilung:* Odstrčil: *Kurze Anleitung zum Rechnen mit den Hamilton'schen Quaternionen*, 1879.

Archiv der Mathematik und Physik. Leipzig, Koch.

LXIX. Band, pp. 1–18. Graefe: *Einige Sätze über abwickelbare Flächen abgeleitet mit Hülfe der Quaternionen.*

American Journal of Mathematics, Johns Hopkins University, Baltimore.

II. 1879, pp. 205–210: W. I. Stringham: *The Quaternion Formulæ for Quantification of Curves, Surfaces, and Solids, and for Barycentres.*

IV. 1881. Beside Prof. Peirce's article: *Linear Associative Algebra*, included in Prof. Tait's list, this volume contains Prof. W. I. Stringham's article, *Determination of the Finite Quaternion Groups*, pp. 345–357.

VI. 1884, pp. 1–13: E. W. Hyde: *Calculus of Direction and Position;* and pp. 270–286: J. J. Sylvester: *Lectures on the Principles of Universal Algebra.*

VII. 1885, pp. 294–326: Arthur Buchheim: *A Memoir on Bi-quaternions.*

American Academy of Arts and Sciences: Proceedings, January 9, 1878: Lowell: *Surfaces of the Second Order as treated by Quaternions* (28 pages octavo).

Giornale di Matematiche, diretto dal Prof. G. Battaglini; Pellerano, Napoli.

Vol. XX. contains a paper by Dino Padelletti, entitled *Principii della Teoria dei Quaternioni elementarmente esposti* (47 pages).

Extract from the introductory Section:—"Il calcolo dei Quaternioni mi sembra in fatti degno di essere maggiormente conosciuto ed apprezzato fra noi: esso fornisce un mezzo efficace per le ricerche geometriche e meccaniche, specialmente adesso che i metodi geometriche tendono a prevalere sempre più nel campo della Meccanica razionale, l'algoritmo hamiltoniano sembra naturalmente indicato per tradurre i teoremi meccanici in formule concise e di facile interpretazione."

"The Calculus of Quaternions seems to me, in fact, worthy of being more known and appreciated among us; it furnishes an instrument efficacious in carrying on geometrical and mechanical researches, and especially now that geometrical methods tend to prevail more and more in the field of rational Mechanics, the Hamiltonian algorithm seems naturally indicated for the expression of mechanical theorems in formulæ at once concise and of easy interpretation."

Professor Padelletti mentions some studies in Quaternions: all of them have been named above except the three which here follow:—

Laisant: *Applications mécaniques du calcul des quaternions.* Paris, 1877.

Bellavitis: *Calcolo dei Quaternioni di Hamilton e sua relazione con il metodo delle equipollenze*, 1858, published in the *Atti della Societa Ital. d. Scienze*, Ser. 2, Tom. i., p. 126.

Prof. Legnazzi: *Aggiunte illustrative alla commemorazione del prof. conte Giusti Bellavitis*, Padova, 1881. Chapter IV. (pp. 181–227) deals with Quaternions.

Clifford's *Mathematical Papers* (Macmillan, London, 1882) contains towards the close exercises on Quaternions.

[In the *Introduction* by Prof. Henry J. Stephen Smith, at pp. lxii.-lxvi., will be found some estimate of the comparative merits of the *Ausdehnungslehre* of Grassmann and the Quaternions of Hamilton. For Hamilton's own statement of the position of Quaternions in reference to the *Ausdehnungslehre* see LECTURES ON QUATERNIONS, Preface (p. 62).]

A. S. Hardy, Ph. D.: *Elements of Quaternions*, second edition, revised, 240 pp., 8vo.

[Dr. Hardy is Professor of Mathematics at Dartmouth College, U.S.A. "The chief aim has been to meet the wants of beginners in the class-room."]

Elementos de Calculo de los Cuaterniones y sus Aplicaciones principales á la Geometria, al Análisis, y á la Mecanica, por Valentin Balbin, Doctor en Ciencias fisico-matematicas, Catedratico di matematicas superiores en la Universidad Nacional di Buenos Ayres, &c.; Buenos Ayres, 1887.

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